# SCIENCE

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CONTENTS:




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## THE DISTRIBUTION OF VACATIONS AT AMERICAN UNIVERSITIES.

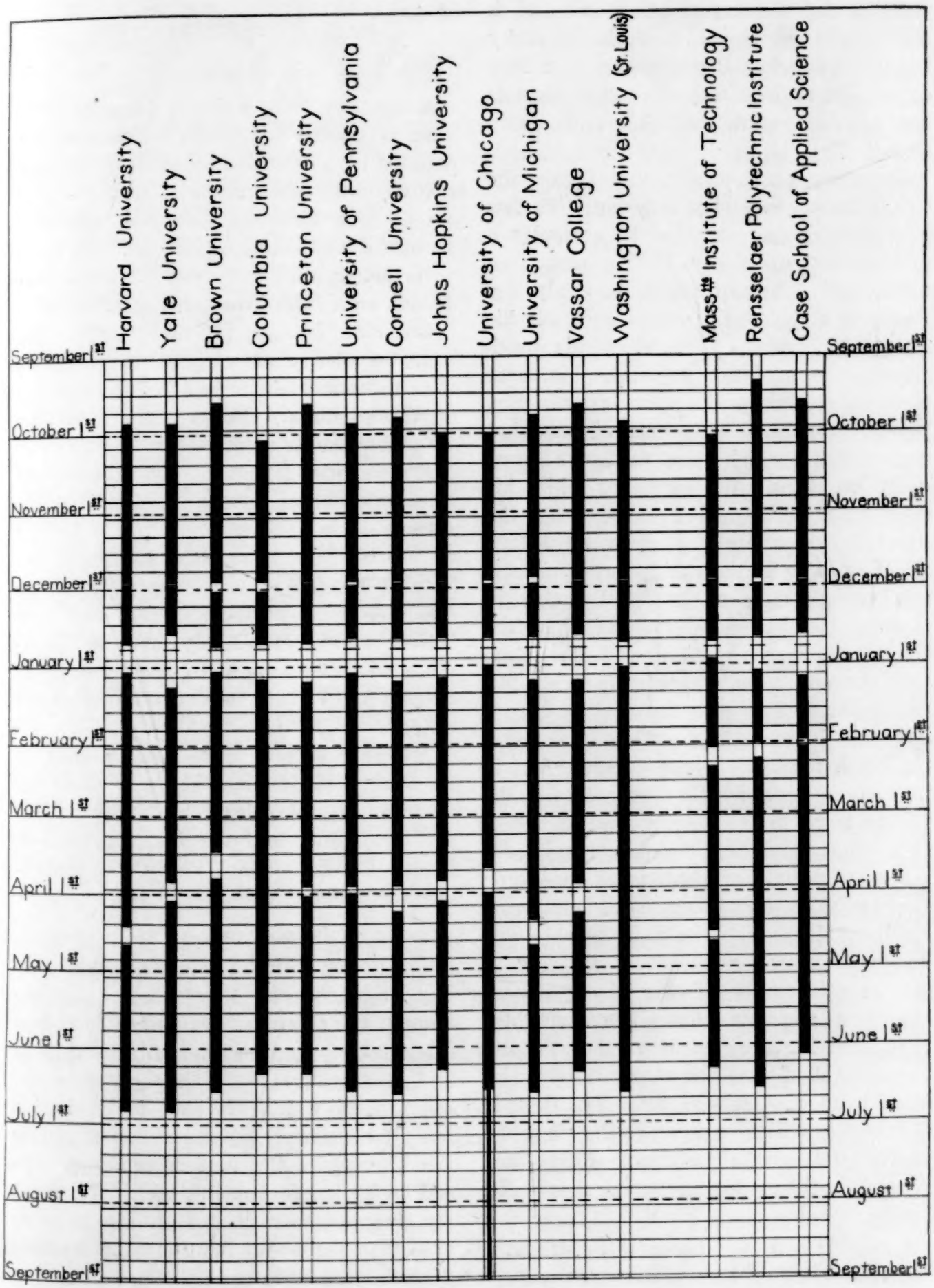
IN connection with the work of the committee appointed by the American Association for the Advancement of Science for the establishment of Convocation Week, it seemed desirable to gather some more accurate impression as to the periods of vacation now in vogue at our universities and colleges. It is very difficult from the reading of catalogues or even by the tabulation of days to gather a clear impression of the differences in the academic calendars of various institutions, so that, almost necessarily, resort was taken to a graphic representation of the facts. The accompanying diagram is perhaps sufficient to indicate the main conditions. In this diagram each vertical column represents the calendar year of one institution. The horizontal lines indicate divisions by weeks. The dotted lines indicate the first day of each month of the academic year from September 1, 1901, to August 31, 1902. In each vertical column the black spaces correspond to term time and the white spaces to vacations. As in every case Christmas falls in vacation time, its position is marked by a black line crossing each column. Fifteen institutions are represented in the chart; twelve universities and three technological schools. The selection has been made so as to have as representative a variety as possible.

Examination of the diagram shows at once the entire absence of any uniformity in the practice of our universities as to the make-up of their calendar. Each institution seems to be a law unto itself, and except that they all begin in the latter part of September or the earlier part of October; that they all have a Christmas vacation and that they all end the year's work in June, there is no uniformity of practice. But even within these broad limits there is great variety. Three of the universities, for instance, begin the third week in September; four begin the fourth week in September; and three the first week in October. In the duration of the Christmas vacation there is even greater proportionate variety, the length being from one week to three. In regard to spring vacations a still greater diversity occurs, some of the institutions having none, one having a considerable vacation in the middle of March, another having a considerable vacation in the middle of April. The length of vacation varies from nothing up to ten days. Equally conspicuous is the discordance of the time of closing the work of the year. The practice of Chicago University of partially continuing the work of instruction during the summer time differs from that of other universities, and this is indicated in the diagram by representing the summer period for that university as half black and half white.

An examination of these diagrams must, I think, convince every one that it is a practical problem for our universities to consider whether they should not aim at a greater or perhaps even a complete uniformity in the length of their term times and of their vacation times. There would be many advantages in this and it would be one of the factors which might contribute to facilitate the migration of students from one university to another. Now that we have an association of a number of the leading universities of the country, the machinery for the proper discussion of the length of the university terms already exists, and it is to be hoped that this association will take up the consideration of this problem, study it thoroughly and, if it seems expedient, introduce a reform in the practice as it now exists.

The national movement in favor of Convocation Week is distinctly in favor of uniformity in the distribution of vacations. It seems to me that the success of the movement in behalf of Convocation Week is complete and that all of our universities will ultimately conform their Christmas vacation to this new demand, as so many of our universities have already done.

Another fact which is very striking in a graphic representation is the very large proportion of time which is wholly given up to vacation. In this respect university students are privileged beings, having a degree of liberty, of exoneration from responsibility and work such as is accorded to young men in no other occupation whatever. In old times when our colleges had, not young men, but boys as students, this great amount of vacation was certainly justified as a measure for the protection of the health of growing boys. But why students of advanced years should be treated as if they were incapable of doing the amount of work which they would have to do if they went into any practical business or profession in life, is not clear. I think it may be safely said that from the standpoint of the benefit to students, the amount of university vacation is very excessive, and when we think how much our young men are now retarded when they go through college and still more if they go through a college and a professional school before they can enter on practical life, we must look upon the


Harvard University
Yale University
Brown University
Columbia University
Princeton University
University of Pennsylvania
Cornell University
Johns Hopkins University
University of Chicago
University of Michigan
Vassar College
Washington University (St. Louis)
Mass# Institute of Technology
Rensselaer Polytechnic Institute
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great extent of vacation as an evil. With the vacation shortened it would be easily possible to bring these young men into active life at least a year earlier than is now possible, and that would be an immense gain.

From the professors' point of view the circumstances are very different. To few professors is the vacation time wholly a period of vacation. On the contrary, it is a time which he can utilize for study, for research and for the increase of all his mental equipment upon which his proficiency as a professor depends. Moreover there is often work upon examination papers or upon a committee of one kind or another, which the professor must carry on during the vacations while his students are idling. For the professor the vacation is certainly a great advantage and I think from the standpoint of university service it is an essential factor in maintaining his efficiency. If therefore the vacations are shortened, it seems to me that every university should provide additional liberty for its professors. The tendency has hitherto been rather to demand too much teaching from professors, but if they teach too much they cannot be qualified to teach in the best manner and with the greatest efficacy, because every professor, to remain efficient, must have time for study; he must advance, he must grow intellectually, and from mere teaching he can never grow.

A consideration of the circumstances therefore suggests these two thoughts: first, that for the benefit of the students the amount of vacation at our universities should be diminished; secondly, that if this is done, then, to preserve the efficiency of the professors, the amount of free time accorded them during term should be increased.

Charles S. Minot.

Harvard Medical School, March 5, 1902.

## *THE INTELLECTUAL CONDITIONS FOR THE SCIENCE OF EMBRYOLOGY.*

### I.

Much has been written, from time to time, about the conditions which must be fulfilled by a scientific account of the generation and regeneration of living things out of eggs, although little has been said about the intellectual conditions. We may, nevertheless, find the study of these conditions both interesting and instructive.

Our chief interest in embryology is the hope for a scientific account of ourselves; but we cannot undertake to account for anything unless we know what it is that we undertake to account for.

My purpose is not to give a scientific account of mind, nor to discount the future progress of science. I do not believe we are likely to know anything about the natural history of mind except what we find out by scientific discovery; nor do I believe we are ever likely to have a complete scientific account of anything, or to reach a point where no new discoveries are needed.

My purpose is a more humble one: to do what I can to keep the way clear for progress in embryology, by trying to free my own mind, and the minds of others, from all notions which imply that embryological science is impossible.

#### Part I. The Doctrine of the Chasm.

The notion which, for reasons which will soon be made clear, I have called the doctrine of the chasm, is dry and difficult and unattractive, and as my only aim is to find a way for the embryologist across the chasms which are said to lie in his path, I have made no attempt to stimulate the interest of the reader, confining myself to the briefest outline which will serve my purpose, even if this outline be more arid than the works in which the doctrine of the chasm is defended.

1. *Among the things of which he hopes to, some day, give an account, the embryologist must include men who think and act.*

Of all the facts that are made known by experiments upon the generation and regeneration of living things, the one we are least likely to doubt is the existence of the experimenter. We may question the value of his results, but we are not likely to doubt that he did, or tried to do, or thought that he did, the things he describes.

The experimental embryologist comes out of an egg, and he must himself be included among the facts of development which are the object of the observations and experiments and reflections by which he seeks to account for the production of living things out of eggs.

Since some of the things that come out of eggs observe, and reflect, and try experiments, the production of living things out of eggs cannot be adequately explained, or accounted for, unless the production out of eggs of things that observe and reflect and try experiments is also explained or accounted for. To make good its claims to our favorable consideration, embryological science must undertake to account, in good time, for minds, in exactly the same sense of the word as that in which it undertakes to account for bodies and brains.

2. *The intellectual powers by the aid of which we make scientific discoveries come out of eggs.*

Honesty, and independence, and accuracy, and determination, and good sense, are essential to sound progress in scientific discovery. The investigator who is no biologist may take his own honesty, and independence, and accuracy, and determination, and good sense, for granted, as ultimate facts that do not need to be accounted for. But honest men, and accurate men, and independent men, and resolute men, and men with good judgment, all come out of eggs, and the embryologist cannot forget that they are among the natural phenomena of which he hopes to, some day, give a scientific account.

The final and decisive test of any explanation of the generation of living things out of eggs is the account which it gives of the origin and significance of our ability to observe and reflect and try experiments; for no scientific discovery is worthy of confidence, unless our intellectual means for finding out things are sound and trustworthy. Thus, the progress of embryological science must bring us around, sooner or later, by a new path, to the old question: What is science? What is it to know a thing? It may be that we shall find, from this new point of view, something in knowledge that has been neglected, or too little considered, and we may thus be helped to better notions.

3. *No embryologist can, knowingly, hold any opinion which excludes the possibility of embryological science.*

Each student of science must devote himself to some small part of the realm of nature in order to make progress. We study simple phenomena in the hope that we may pass, in time, to those that are more complex and difficult. If astronomers, and chemists, and students of physics, and embryologists, and zoologists, see fit to temporarily lay aside the natural history of mind, as a problem which does not, for the time, interest them, nor seem to concern them, or as something that is too hard for them, no one can doubt their wisdom. But if their methods and results lead them, or seem to lead them, to the conclusion that what has thus been temporarily laid on the shelf can never be taken down from the shelf, is it not clear that there has been a mistake somewhere? Any method of embryological research which leads to the conclusion that there is a 'chasm' which is 'intellectually impass-

able,' between the facts of embryology and the facts of consciousness, is self-condemned, because it denies the possibility of a science of embryology. Any method of embryological research which leads to the conclusion that the phenomena of consciousness are not phenomena at all, but 'epiphenomena,' and the mere empty and meaningless accompaniment, or by-product, of phenomena, is self-condemned; because the phenomena of knowledge—of embryology, and of everything else—are phenomena of consciousness.

4. *Many eminent authorities tell us an embryological account of human minds is impossible.*

It is well known that many writers, who claim to speak of the meaning of modern science with authority, have been led to believe that the facts of consciousness can never be brought back into the system of science.

Thus, for example, Tyndall tells us: "The passage from the physics of the brain to the corresponding facts of consciousness is unthinkable. Granted that a definite thought and a definite molecular action in the brain occur simultaneously, we do not possess the intellectual organ, nor apparently any rudiment of the organ, which would enable us to pass by a process of reasoning from the one phenomenon to the other. They appear together, but we do not know why. Were our minds and senses so expanded, strengthened and illuminated, as to enable us to see and feel the very molecules of the brain; were we capable of following all their motions, all their groupings, all their electrical discharges if such there be; and were we intimately acquainted with the corresponding states of thought and feeling, we should be as far as ever from the solution of the problem, How are these physical processes connected with the facts of consciousness? The chasm between the two classes of phenomena would still remain intellectually impassable."

If for *brain* we put *egg which gives rise to a brain,* this statement must mean one of two things: Either there is a chasm, which is intellectually impassable, between the facts of embryology and the facts of consciousness; or else there are two sets of embryological facts—physical and psychical—separated by the impassable chasm; and, therefore, two equally independent and distinct sciences of embryology. Tyndall cannot admit that the facts of physics may have their being in a knowing mind, for, in this case, there would not be any chasm.

Professor James, who is also a believer in the chasm, tells us there is a 'link' or bridge, but as he also tells us 'we do not know, and most authorities believe we never shall know, and never can know,' what the link is, or where the bridge is, neither link nor bridge is of much practical use to embryologists.

According to the system of scientific philosophy which finds expression in these extracts, the embryologist may hope to pass from the physics of atoms and molecules and organic matter, to physical eggs and physical men; and, if there be any psychical atoms and molecules and compounds, he may hope to pass from them to psychical eggs and psychical men; but the chasm between the sort of eggs we know and the sort of men we know is intellectually impassable.

Herbert Spencer, who is held to be the philosophical spokesman of modern science, is also a believer in the chasm; and he tells us that mind is 'something without any kinship with other things; and from the sciences which discover, by introspection, the laws of this something there *is no passage* by transitional steps to the sciences which discover the laws of these other things.'

We may pass, by a process of reasoning, from a physical candle to a physical burn, and, if this system of philosophy is to be trusted, we might, if we knew a psychical candle, pass from it to a psychical pain, but we can never pass from a physical candle to a psychical pain by any intellectual process, nor know a burn hurts in the way we know a flame burns.

5. *The chasm is not an easy thing to understand.*

Many questions are too hard for us, for we are very ignorant, and we have only feeble and incomplete command of the scientific method of finding out things; but these familiar truths are not what Tyndall and Spencer have tried to express in the passages I have quoted. These passages are no humble confession of ignorance. They are very positive assertions that something—an intellectual grand canyon—is very definitely known. We are told that we know—know with certainty—that the method which is used in physical discovery is fundamentally and utterly inadequate for dealing with the relation between bodies and minds—utterly inadequate for dealing with the relation between eggs and the thinking men who come out of eggs. The grand canyon is not merely difficult. It is utterly impassable.

6. *We are told that there is a chasm, because I cannot know the minds of other men in the way I know my own mind.*

Among the reasons which are given for belief in the chasm, the simplest is my alleged inability to know the minds of other men in the way I know my own mind. But I can never know my own body in the way I know the bodies of other men. I can have no more immediate perception that there is in my head a sphenoid bone which has arisen, during my younger stages, through the union of a presphenoid, a basi-sphenoid, two ali-sphenoids, two orbito-sphenoids, and two pterygoids, than I can have immediate perception that there is in Timbuctoo a man with a mind as much like my mind as my body is like his body. My conviction that I have passed through embryonic stages like those described in the text-books is even more remotely inferential than my conviction that my own familiar friend has a mind like mine.

The chasm between my embryonic history and that of other human beings is utterly impassable, yet its impassability is practical and not intellectual. I find no more logical difficulty in believing that I could perceive the resemblance between my brain, or my embryonic history, and those of other men, if I were in the proper place at the right time with suitable means of observation, than I find in the belief that I could thus perceive the other side of the moon.

If there is a grand canyon, it must be of a different sort from the chasm between my body and those of other men, for this is not intellectual, but practical.

7. *There is a chasm, we are told, because I know my own mind by introspection.*

It is, unquestionably, through introspection that I know my own mind and this is the reason why we are told that there is an impassable chasm between mind, on the one side, and brains and all other material things, on the other.

A moment's reflection is enough to show that it is through introspection—through comparison of my sensations, and recollections, and expectations, and other mental facts, and through reflection upon them—that I find out anything. If I neither felt nor reflected, I should not know anything. It is through reflection upon my thoughts and feelings that I make scientific discoveries about my mind, and about the minds of other men, and about everything that I know. As I have only this one way to find out anything, it is hard to imagine

where the impassable chasm is, but what chiefly concerns us now is the wide diffusion of belief in its reality.

8. *The chasm is said to be between the things I may know, or might know, and something unknowable.*

The chasm cannot be between my mind and anything I know, or may know, or might know if I had the opportunity, because the things I know are in my mind, and I can never know anything except knowledge. So we are told that it is between things that are knowable and something that is not only unknown and unknowing, but unknowable.

Believers in the chasm do not all put it in the same place. Some declare that we know nothing but the molecular or electrical changes in our ganglion cells. Forgetting the existence of their own thoughts, or else dismissing them as mere 'epiphenomena,' without significance, they tell us that the chasm is between these physiological changes and the real world to which we try to refer them.

We have no immediate knowledge of our own brains, but we do know the thoughts that arise in our minds, and Tyndall tells us the chasm is not between the physiological changes in our brains and the facts of physics, but between thoughts and the physiological changes in our brains.

A third, and, on the whole, a more consistent notion, is that we know *impressions*, but can never know the thing impressing, nor the thing impressed, nor whether the thing impressing and the thing impressed are two different kinds of unknowables, or only two unknowables of the same kind. This is Spencer's opinion, as I understand it, and it is the opinion of many scientific men.

We know phenomena, or appearances, they tell us, but are altogether put off with appearances, and can never know either things or minds as they are in themselves. We know the eggs in our minds, and the hens in our minds, but as for knowing eggs as they are in themselves or hens as they are in themselves, that, we are told, is forever out of our reach on the other side of the chasm. We may know the human ovum in our minds and the thinking man in our minds, but the human ovum as it is in itself and the thinking man as he is in himself, are utterly unknowable.

When the fact that we know the hens in our minds is joined to the notion that our minds are in our heads, we reach the interesting, but startling, opinion, that the hens we know are the hens inside our heads. Efforts to escape this strange admission by the assertion that we know only the appearance, and not the reality, of hens in our heads, lead one to suspect that the intellectual chasm may not be a grand canyon after all, but only a common bog in which the wayfarer is the more completely mired by his own struggles.

He who believes he can never know anything as it really is, can never know whether what he thinks he believes or disbelieves is really what he thinks it is, rather than something quite different; so the question whether he can believe or disbelieve anything is not without interest, although we need not go into it now.

9. *The chasm is not between the things we know and the things that remain to be known.*

The embryologist is well aware that he cannot hope to find out all there is to learn about hens' eggs, or about his mind, or about anything else; but he attributes this truth to the boundless wealth of nature, and not to any inherent weakness in his methods. In this meaning of the words, he has no expectation, and no hope, that he will ever know a hen's egg as it really is; and if the chasm were only between the things he knows and the things he has not yet found out, he would frank-

ly and humbly admit its existence and its practical impassability. But it is said to be a chasm between things knowable and things utterly and absolutely unknowable, and not a chasm between the things that are known and the things that remain to be known.

The translator of Haeckel's 'Riddles of the Universe' tells us in his preface, that the chasm has been devised by the Roman Catholic theologians for their own evil ends, but it is not kind to lay upon the backs of these heavy-laden and weary creatures a burden which Tyndall and Spencer and others have shown themselves so eager to bear with jaunty dexterity.

It is true that the slow and heavy intellect of the embryologist cannot aspire to the subtile agility which some show in dodging chasms.

"And now," says the author of 'Father Tom and the Pope,' "I have to tell you ov a really onpleasant occurrence. If it was a Prodesan that was in it, I'd say that while the Pope's back was turned, Father Tom made free wid the two lips of Miss Eliza."

"It is kissing my housekeeper before my face you are, you villain?" says he. "Go down out of this," says he to Miss Eliza; "and do you be packing off wid you," says he to Father Tom, "for it's not safe, so it isn't, to have the likes of you in a house where there's temptation in your way."

"Is it me?" says his Riv'rence; "why what would your Holiness be at, at all? Sure I wasn't doing no such thing."

"Would you have me doubt the ividence ov my sinses?" says the Pope; "would you have me doubt the testhimony ov my eyes and ears?" says he.

"Indeed I would so," says his Riv'rence, "if they pretend to have informed your Holiness of any such foolishness."

"Why," says the Pope, "I'v seen you afther kissing Eliza as plain as I see the nose on your face; I heard the smack you gave her as plain as ever I heard thundher."

"And how do you know whether you see the nose on my face or not?" says his Riv'rence, "and how do you know whether what you thought was thundher, was thundher at all? Them operations on the sinses," says he, "comprises only particular corporal motions, connected wid sartain confused perciptions called sinsations, and isn't to be depended upon at all. If we were to follow them blind guides we might jist as well turn heretics at onc't. 'Pon my secret word, your Holiness, it's neither charitable nor orthodox to set up the testimony of your eyes and ears agin the characther ov a clergyman. And now see how aisy it is to explain all them phewnomena that perplexed you. I ris and went over beside the young woman because the skillit was boiling over, to help her to save the dhrop of liquor that was in it; and as for the noise you heard, my dear man, it was neither more nor less nor myself dhrawing the cork out of this blissed bottle."

"Don't offer to thrape that upon me!" says the Pope; "here's the cork in the bottle still, as tight as a wedge."

"I beg your pardon," says his Riv'rence, "that's not the cork at all," says he, "I dhrew the cork a good two minutes ago, and it's very purtily spitted on the end of this blessed cork-schrew at this prisint moment; howandiver you can't see it because it's only its real prisence that's in it. But that appearance that you call a cork," says he, "is nothing but the outward species and external qualities of the cortical nathur. Them's nothing but the accidents of the cork that you'r looking at and handling; but, as I tould you afore, the real cork's dhrew, and is here prisent on the end of this nate little

insthrument, and it was the noise I made in dhrawing it, and nothing else, that you mistook for the sound of the *pogue.*"

You know there was no conthravening what he said; and the Pope couldn't openly deny it. Howandiver he thried to pick a hole in it this way.

"Granting," says he, "that there is the differ you say betwixt the reality of the cork and these cortical accidents; and that it's quite possible, as you allidge, that the threw cork is really prisent on the end of the schrew, while the accidents keep the mouth of the bottle stopped—still," says he, "I can't onderstand, though willing to acquit you, how the dhrawing of the real cork, that's onpalpable and widout accidents, could produce the accident of that sinsible explosion I heard jist now."

"All I can say," says his Riv'rence, "is that it was a rale accident any how."

"Ay," says the Pope, "the kiss you gev Eliza, you mane."

"No," says his Riv'rence, "but the report I made."

"What makes you call the blessed quart an irrational quantity?" says the Pope.

"Because it's too much for one and too little for two," says his Riv'rence.

"Clear it of its coefficient, and we'll thry," says the Pope.

"Hand me over the exponent then," says his Riv'rence.

"What's that?" says the Pope.

"The schrew, to be sure," says his Riv'rence.

"What for?" says the Pope.

"To dhraw the cork," says his Riv'rence.

"Sure the cork's dhrew," says the Pope.

"But the sperets can't get out on account of the accidents that's stuck in the neck of the bottle," says his Riv'rence.

"Accident ought to be passable to sperit," says the Pope, "and that makes me suspect that the reality of the cork's in it afther all."

"That's a barony-masia," says his Riv'rence, "and I'm not bound to answer it. But the fact is, that it's the accidents of the sperits, too, that's in it, and the reality's passed out through the cortical species as you say; for, you may have observed, we've both been in real good sperits ever since the cork was dhrawn, and where else would the real sperits come from if they wouldn't come out of the bottle?"

"Well, then," says the Pope, "since we've got the reality, there's no use in throubling ourselves wid the accidents."

"Oh, begad," says his Riv'rence, "the accidents is very essential, too; for a man may be in the best of sperits, as far as his immaterial part goes, and yet need the accidents of good liquor to hunt the sinsible thirst out of him."

10. *The assertion that each thing has a mind of its own is irrelevant.*

One way of rescuing science from the dilemma of the chasm, which has the approval of many modern students, is to assert that every living thing, or every thing, has its own mind, and does what it does because it chooses; and that eggs and candles are, in fact, psychical eggs and psychical candles.

"Call an organism a machine if you will," says Professor Ward in his recently published Gifford Lectures, "but where is the mind that made it, and, I may add, that works it?" And he answers his question by the assertion (I., p. 294) that the mind that makes the living organism is inside it and identical with it, and that every living thing takes conscious and efficient part in its own production. The context shows that Ward believes it is as a conscious and voluntary agent, and not merely as a part of an intended system of nature, that the hen's egg is said to help to make itself into a chick. The mind that

is said to be 'inside it and identical with it' is an individual and particular mind, and not the *anima mundi,* nor the mind that presides over the universe. The notion that each thing has its own mind, or is a mind, has nothing in common with the opinion that it is in one sustaining mind that we and all things have being.

The notion that now concerns us reaches its logical culmination in Major Powell's assertion that "Every body, whether it be a stellar system or an atom of hydrogen, has consciousness as judgment and choice." If a hen's egg would describe to us the way in which it makes a chick, I should be delighted to listen and learn from it; but, until it does, embryologists must struggle along in the old-fashioned way.

If each thing has its own mind, and is identical with it, there is, of course, no chasm, because we are really studying psychology, when we think we are studying physics. But this way of escape from the chasm leads us into new difficulties, which are just as impassable as the chasm, and very much more practical.

Even if we admit that the hen's egg may have, or be identical with, a mind as good as a hen's mind, the hen's body is so fearfully and wonderfully made that the wisest man, whose mind is assuredly better than a hen's mind, is at present utterly incompetent to make, or even to understand, a hen. If it is by wisdom that hens are made, it must be by a higher wisdom than a hen's, for this cannot attain to such a work.

It is not by studying the consciousness of atoms and molecules, and material things, that we have found out how to make chemical compounds, and machinery and books; and if we are ever to find out how to make living eggs, one may safely predict that it will not be through the study of the judgment and choice of the eggs of sea-urchins and frogs and hens.

Haeckel, who declares that Berkeley, of all men, believed that 'one thing only exists, and that is my own mind,' also tells us of his own belief that "the two fundamental forms of substance, ponderable matter and the ether, are not dead and only moved by extrinsic forces, but they are endowed with sensation and will (though, naturally, of the lowest grade); they experience an inclination for condensation, a dislike of strain; they strive after the one, and struggle against the other." Only they know nothing about it, for Haeckel tells us: "I conceive the elementary psychic qualities of sensation and will which may be attributed to atoms, to be unconscious." Still, while they do not know it, "every shade of inclination, from complete indifference to the fiercest passion, is exemplified in the chemical relation of the various elements towards each other, just as we find in the psychology of man, and especially in the life of the sexes. This fundamental unity of affinity in the whole of nature, from the simplest chemical process to the most complicated love-story, was recognized by the great Greek scientist, Empedocles, in the fifth century B. C., in his theory of 'the love and hatred of the elements.' It receives empirical confirmation from the interesting progress of cellular psychology, the great significance of which we have only learned to appreciate in the last thirty years. On these phenomena we base our conviction that even the *atom* is not without a rudimentary form of sensation and will."

Words are democratic, and one is, intrinsically, as good as another. What common folks call *things,* may be called *minds,* or *abracadabra,* or $x$, by any one who so chooses, provided he know what he means, and make himself understood; but if he thinks that, by calling *things minds,* he can find out anything which would not be

within his reach if he called them *x*, he seems to me to be misled by words.

As an explanation of the generation of chicks from hens' eggs, the fantastic and pantheistic animism of the passages I have quoted is irrelevant and useless, and no student of Berkeley's works, whether his frame of mind be critical or responsive, can confuse it with the sublime conviction of this thinker that it is in one sustaining mind that we and all things have being.

12. *Belief in the chasm may be due to some error in the description of the way in which we find out things.*

There are no paradoxes nor contradictions in nature. When facts seem to contradict one another, better knowledge is continually showing that some mistake has been made. If physical science leads us, or seems to lead us, to the belief that the chasm between an egg and the thinking man who comes out of an egg is intellectually impassable, the embryologist must ask where the mistake is.

It is a hard thing to believe that, beneficial and good as science has shown itself to be, it can lead us into opinions which cannot be maintained and made consistent. Science is justified in her works, and I find it hard to believe that the paradox of the chasm can be due to the method in which discoveries are made, or that this method can involve us in contradictions, and lead to intellectual disaster.

On the other hand, it is not a hard thing to believe that there may be some error or omission in the account which successful scientific investigators give of their method. He who reflects upon the perplexities which come from the misuse of words will find it an easy thing to believe that an account of the way in which things are found out may be so imperfect that it is practically equivalent to error, leading those who try to find out things by following it into contradiction and absurdity. It may be that the philosophical spokesmen of science have been drawn into paradoxes and contradictions and doubt of the plainest things, because they have mistaken some crude and imperfect account of the way in which we find out things for the way in which we really do find out things. There may be, in knowing, something so familiar and obvious that it is commonly left out of the description of the process of knowing.

13. *We are told that we know things when we comprehend them, but knowledge may be comprehension and something more.*

The eloquent plea for science, as a guide to conduct, with which the author of a new 'Grammar of Science' begins his book, must strike a responsive chord in the mind of every student of nature.

"Apart," he says, "from the increased physical comfort, apart from the intellectual enjoyment which modern science provides for the community, there is another and more fundamental justification for the time and material spent in scientific work. From the standpoint of morality, we have to judge of each human activity by its outcome in conduct."

Something in my own mind vibrated in harmony with the author's words as I read; but, as he is soon led, by his definition of science as *the analysis and classification of facts,* to believe and to teach that our conduct is nothing but a *routine,* over which we have no real control, and for which we have no true responsibility, his premises seem to compel me to look at his book from the *standpoint of morality,* and to *judge of his intellectual activity by its outcome in conduct.*

I am puzzled, in my attempt to do this, by a moral question about the publication and sale of this book. My difficulty is this. The author's definition of science, as the analysis and classification of facts, leads

him to believe, and to teach, that "the universal validity of science depends upon the similarity of the perceptive and reasoning powers of normal civilized men." A writer on the meaning of science, whose name does not appear in our author's bibliography, showed, some two thousand years ago, that the sale of this opinion for money is not honest; for if the verdict of civilized men be the criterion of science, the way to find out what nature really is must be by ballot. This old writer therefore says that our author is disingenuous when he asks us to buy and read his book in the hope of learning something which he is not able to deliver to his customers, since he himself believes we can get it only through the verdict of civilized men. If the 'Grammar of Science' is anything more than a ballot, I see no way to acquit its author of the charge of obtaining money under false pretenses.

Has not the merest savage a criterion of science which will bear him up though all men be against him? May he not appeal to nature in the same confidence that he will bring to his side all normal civilized men who do not wilfully turn away their eyes?

Herbert Spencer, who also tells us knowledge is the analysis and comprehension of facts, tells us, furthermore, that this is one of the proofs that we can never know anything as it really is, because the thing as it really is is separated, by an impassable chasm, from the appearance which is all we can know.

"For if the successive deeper interpretations of nature which constitute advancing knowledge are merely the inclusion of special truths in general truths, and of general truths in truths still more general, it obviously follows that the most general truth, not admitting of inclusion in any other, does not admit of interpretation. Of necessity, therefore, explanation must eventually bring us down to the inexplicable. The deepest truth we can get at must be unaccountable. Comprehension must become something else than comprehension, before the ultimate fact can be comprehended."

We undoubtedly comprehend a thing when we know it, but it does not follow that we know a thing when we comprehend it. The conclusion does not follow from the premises. Knowledge may be comprehension and something more, and the assertion that comprehension is knowledge, as well as all the books of synthetic philosophy that are built upon this assertion, may, perhaps, turn out to be nothing more than a new illustration of the fallacy of the undistributed middle.

14. *Knowledge must be something more than comprehension, because the known world grows with knowing.*

Here I must stop, for the present, leaving for some future occasion the attempt to find out, in the interest of embryological science, whether this account of knowing is, or is not, complete. But, before I end, I ask you to take away with you, and to consider, this familiar truth: Each scientific discovery shows us new and unsuspected wonders in nature. The unexplained things which are brought to our knowledge by each scientific explanation far outnumber the things it explains. The progress of knowledge is no mere comprehension, or gathering in. It is more like sowing seed than gathering a harvest, for the known world grows with knowing.

We are told that "when every fact, every past or present phenomenon of the universe, every phase of present or past life therein, has been examined, classified, and coordinated with the rest, then the mission of science will be complete." But if we are to judge the future by the past, classification and coordination will always continue to show us more unclassified and uncoor-

dinated things than they classify and coordinate.

May it not be because of the inexhaustible bounty of nature, and not because comprehension is knowledge, that we can never know anything as it really is?

Each new encyclopedia is bigger than the one before, and so, no doubt, it will be to the end. If knowledge were nothing more than comprehension, or the analysis and classification of facts, the progress of science should be bringing us nearer to universal knowledge, but each new discovery puts it farther from our grasp than before, and they who know most are most convinced of its unattainableness, not because the reality of things is unknowable, but because of the innumerable multitude of things knowable.

W. K. Brooks.

Johns Hopkins University.

## *THIRD ANNUAL MEETING OF THE BOTANISTS OF THE CENTRAL STATES.**

FIRST SESSION, HULL BOTANICAL LABORATORY, ROOM 13, TUESDAY, 9 A.M.

The meeting was called to order by C. R. Barnes. About seventy botanists were present. J. M. Coulter was elected chairman and Albert Schneider secretary. After a few preliminary remarks the chairman called for the reading of scientific papers, which were presented as follows:

Charles F. Millspaugh: 'The Clothing of an Islet.' (No abstract furnished.) Illustrated by lantern slides.

George H. Shull: 'Variations in Several Species of *Aster*.' Counts were made of bracts, rays and disk florets in *Aster Shortii* Hook., *A. Novæ-Angliæ* L., *A. puniceus* L., and *A. prenanthoides* Mühl. The result of these counts gave but a single instance of a maximum falling on a member of the Fibonacci-series, 3, 5, 8, 13, 21, etc., the rays of *Aster Shortii* presenting a strong mode on 13; a general result giving but slight confirmation of Ludwig's results on various other Compositæ. The counting of the parts of heads collected on September 27, 30, October 4 and 8, from a single small plot of *Aster prenanthoides*, and comprising collectively all the heads produced in one season, showed, alike in bracts, rays and disk florets, a constant fall in the mean number and a corresponding shifting of the modes from the beginning to the end of the flowering season. This fact must be taken into account in the determination of place modes. There is a close correlation between the number of rays and the number of bracts, due to the fact that the rays are axillary to the inner bracts. In the four species studied the degree of imbrication of the bracts, and also the difference in form and size between the outer and inner bracts of the head are proportional to the number of bracts which bear no rays in their axils. A complete account of these studies will appear in the *American Naturalist* for February, 1902.

Edwin B. Copeland: 'The Influence of Metallic Poisons on Respiration.' Experiments with *Elodea, Callitriche,* a crucifer, fish and frog larvæ, using as stimulants copper, zinc, cadmium, silver and mercury, agree in showing that the respiration may be stimulated by a small fraction of a fatal concentration. With increasing concentration the acceleration of $CO_2$-evolution is greater, sometimes reaching above 25 times the normal. Evolution of $CO_2$ continues undiminished after plasmolysis is suspended by the poison. Copper and zinc cause the evolution of considerable $CO_2$ from boiled *Elodea*.

Frederick C. Newcombe: 'The Sensory Area of the Roots of Land Plants.' In the roots of land plants, sensitiveness to exter-

* Held in connection with the meeting of the American Society of Naturalists, at the University of Chicago, December 31, 1901, to January 2, 1902.

nal stimuli has been considered to be confined to the apex and to the elongating zone. The elongating zone in nearly all species is confined to the first 10 mm. of the apex. In studying the phenomena of rheotropism it was found that the region of the root posterior to the elongating zone is sensitive as well as the elongating zone itself. To determine the location of the sensory tissue, various parts of the root were shielded from the flow of the water by enclosure in glass tubes. The roots of the radish, white mustard, buckwheat, sunflower, and popcorn gave good rheotropic curves when stimulated at a distance of 10 mm. to 15 mm. from the limit of the elongating zone.

FRANCIS RAMALEY: 'Mesa Vegetation.' The plants of these long, gently sloping, flat-topped ridges are distributed in characteristic fashion, depending upon the various edaphic conditions presented by different slopes and exposures. Probably the most interesting feature to be noted is the distribution of trees and shrubs. These plants are present on the tops of the mesas at their western ends, absent farther east because of dryness, present on the north and absent on the south slopes. On the south slopes shrubs occur near the top. This portion of the slope, in most hills the driest, is here somewhat moist because the snow remains late in the spring on the flat tops, and in melting the water trickles down the sides of the hill and is absorbed near the top. There is thus more moisture near the top than farther down, and the occurrence of a fringe of shrubs and trees at the top is thus explained. Illustrated by lantern slides.

D. M. MOTTIER: 'The Behavior of the Chromosomes in the Spore Mother Cells of Higher Plants and the homology of the Pollen and Embryo-sac Mother Cells.' The author discussed the behavior of the chromosomes in the pollen mother cells of the species of *Lilium, Podophyllum peltatum* and *Tradescantia Virginica,* and in the embryo-sac of *Lilium Martagon,* with the following results: The earlier view of Farmer and Strasburger, that a double longitudinal division of the chromosomes takes place during the first mitosis, is confirmed. The second longitudinal splitting takes place in a plane at right angles to the first. This is clearly seen during metakinesis; it may occur earlier. There is, therefore, no longitudinal fission of the chromatin spirem during the second mitosis, and no reduction division in the sense of Weismann. In the reconstruction of the daughter nucleus the granddaughter chromosomes unite to form a single chromatin spirem. In *Tradescantia* especially, the granddaughter segments tend to reticulate so that an irregular spirem is the result, and the daughter nucleus approaches the structure of the resting stage. The identity of the individual chromosomes is lost in the daughter nucleus. The first two mitoses in the embryo-sac mother cell are like in character to those in the pollen mother cell, and consequently the micro- and macrospore mother cells are homologous. The type of development of the embryo-sac in which the heterotypic and homotypic mitoses result in four potential macrospores, as for example in *Helleborus,* is regarded as the more primitive, while that found in *Lilium* is considered as a derived form. No case is known in which the pollen mother cell develops directly into the pollen spore. Illustrated by lantern slides.

SECOND SESSION, 2 P.M.

Meeting called to order by the chairman. About seventy-five were present. The chairman read the following message:

**Society of Plant Morphology and Physiology in session at Columbia sends greetings to Botanists of Central States at Chicago.**

ERWIN F. SMITH, *President,*
WILLIAM F. GANONG, *Secretary.*

A return message was ordered sent to President Erwin F. Smith. The secretary was directed to arrange for contiguous seats for botanists at the annual dinner. Charles F. Millspaugh extended a formal invitation to all botanists to visit the Field Columbian Museum.

The reading of papers was resumed:

Conway MacMillan: 'A Marine Biological Station on the Straits of Fuca.' A series of lantern slides were shown illustrating the buildings and surroundings of the Minnesota Seaside Biological Station on the Straits of Fuca. Among the views were a number illustrating the kelp formation of the Vancouver coast and several photomicrographs of the anatomy of *Pterygophora californica* Rupr., a plant upon which particular study had been expended. The excellence of the locality as a field for station activities was pointed out and some of the plans for the coming summer were indicated.

Harold L. Lyon: 'The Phylogeny of the Cotyledon.' Modern researches in angiospermic embryology have shown the prevalent foliar theory of cotyledons to be untenable. A careful survey of the investigations already recorded has led to the following conclusions. (*a*) The typical embryos of the Pteridophytes and Angiosperms differentiate into three primary members—the cotyledon, stem and root. (*b*) Cotyledons are not arrested leaves, but are primarily haustorial organs originating phylogenetically as the nursing-foot in the Bryophytes and persisting throughout the higher plants. (*c*) The monocotyledonous condition is the primitive one and prevails in the Bryophytes, Pteridophytes, Monocotyls and some Gymnosperms. The two (sometimes more) cotyledons of the Dicotyls are jointly the homologue of the single cotyledon of the Monocotyls. (*d*) The cotyledon always occurs at the base of the primary stem. (*e*) The hypocotyl is a structure peculiar to the Angiosperms, being differentiated between the primary stem and root. (*f*) The so-called cotyledons of the Pteridophytes, and Gymnosperms, with the exception of *Ginkgo* and the Cycads, are true foliage-leaves.

E. Mead Wilcox: 'Valvular Torsion as a Means of Seed-dispersal in *Ricinus*.' For the purpose of securing accurate data regarding the efficiency of valvular torsion for seed-dispersal in *Ricinus*, a plant was selected, growing in an open field. The ground about this plant was divided into four quadrants designated, N.E., S.E., S.W. and N.W. The surface was frequently cultivated so that the seeds would not be blown about by winds after falling. The following table shows the distances to which seeds were thrown, measured from the base of the plant. The plant was 104 cm. in height and the inflorescence, at maturity, was 36 cm. in length:

| Distance from Center (cm. . | Number of Seeds. | | | | Totals. |
|---|---|---|---|---|---|
| | N.E. | S.E. | S.W. | N.W. | |
| 0–49 | 9 | 29 | 19 | 11 | 68 |
| 50–99 | 8 | 12 | 16 | 9 | 45 |
| 100–149 | 3 | 7 | 7 | 14 | 31 |
| 150–199 | 5 | 6 | 4 | 3 | 18 |
| 200–249 | 4 | 6 | 4 | 3 | 17 |
| 250–299 | 1 | 2 | 1 | 2 | 6 |
| 300–349 | 0 | 2 | 2 | 0 | 4 |
| Totals. | 30 | 64 | 53 | 42 | 189 |

The greatest distance to which any seed was thrown was 325 cm. On 12 of the 19 days upon which observations were made the wind was from the south.

Cyrus A. King: 'Fertilization and Some Accompanying Phenomena in *Araiospora pulchra*, one of the Aquatic Phycomycetes.' Araiospora has the habit of *Saprolegnia*, growing attached to twigs in water. Both genus and species were established by Thaxter in 1896. The sexual organs resemble those of the Peronosporineæ.

The oogonia when cut off contain about fifty nuclei, which move toward the periphery while the interior is still a coarse cytoplasmic mesh-work. Patches of fine meshed cytoplasm now arise at various places in the oogonium; these later fuse into one central irregular mass which never loses its mesh-like character. This central structure, which corresponds to bodies previously observed in the Peronosporineæ and *Pythium*, reaches its highest development at the time the sperm nucleus enters. Soon afterwards it begins to spread out into the peripheral ooplasm. Just before the separation of egg from periplasm, the nuclei probably all divide once, mitotically. The egg, when ripe, consists of this previously described central area, which now has a female nucleus imbedded in it, surrounded by a coarse, uniformly vacuolate peripheral portion. Enclosing the egg, though sharply marked off from it, is the periplasm which, at this time, is divided anticlinally into a single layer of cells.

No such structure as an antheridial tube was seen. The fertilizing tube is entirely of oogonial origin. The protoplasm in contact with the oogonial wall where the antheridium is appressed, and where the oogonial papilla is developed, always remains with the ooplasm. Consequently, the plasma membrane of the periplasm, as it lays down a wall between it and the ooplasm, builds the wall of the fertilizing tube. As soon as this tube is formed, the perforation is made by the papilla and a sperm nucleus and some cytoplasm are admitted. As the nuclei approach, both put out beaks which, at least in some cases, fuse. When the wall of the oospore is well developed the latter is binucleate. The important points in the paper are: (*a*) Fertilization takes place by the union of a single male and female nucleus. (*b*) An organ of attraction for the sexual nuclei arises in the early development of the oogonium and its origin, structure and fate is followed. (*c*) There is no such fertilization tube as is figured in related forms. The tube here is a conjugation tube and the opening a conjugation pore, as Harper has suggested in *Pyronema*. Illustrated by lantern slides.

FREDERICK DEFOREST HEALD: 'The Electrical Conductivity of Plant Juices.' Using the methods of physical chemistry, conductivity measurements were made for the juice expressed from the leaves, stems, roots, etc., of different plants. The following species were used: *Beta vulgaris, Solanum tuberosum, Allium cepa, Raphanus sativus, Nuphar advena, Cucumis sativus, Amarantus retroflexus* and *Portulaca oleracea*. Ash determinations were also made for the juices used and the ash redissolved in distilled water and diluted up to the original volume of the juice from which it was obtained. Specific conductivity determinations were made for the ash solutions. The following conclusions were drawn from the various determinations. (*a*) Plant juices are comparatively good conductors, the conductivity being due in large measure to the dissolved mineral substances, while the organic compounds play a minor part. (*b*) The specific conductivity of the juice obtained from the roots of plants is always considerably less than that of the juice obtained from the subaerial parts of the plant. (*c*) The specific conductivity generally increases progressively from the root upward, although in some cases the sap from the stem has a higher conductivity than that from the leaves. (*d*) In the majority of cases the specific conductivity is a rough measure of the relative amount of ash present in different parts of the plant. Illustrated by lantern slides.

H. G. TIMBERLAKE: 'Starch Formation in *Cladophora*.' The process of starch formation in *Cladophora* was described as oc-

curring in essentially the same manner as in *Hydrodictyon* (*Annals of Botany*, Dec., 1901). In material killed in various killing fluids, sectioned with a microtome and stained with the safranin gentian-violet orange mixture, stages in the transformation of a portion of a pyrenoid into a starch grain were observed. All the starch grains arise in this manner. There is no distinction in origin between the so-called pyrenoid starch and stroma starch. In these cells starch cannot be said to be the first visible product of photosynthesis, since it is formed from a visible proteid body, the pyrenoid.

B. E. Livingston: 'Influence of the Osmotic Pressure of the Surrounding Medium upon the Growth and Production of Living Organisms.' A change in the surrounding solution may result in either a physical or a chemical change in the solution contained within the organism. By physical change is to be understood a mere change in general concentration, brought about by absorption or extraction of water. A strong solution will extract water from the organism, a weak one will allow it to be absorbed. By chemical change is meant changes caused by absorption or extraction of solute particles. Change in the water content of the protoplasm may be directly effective by causing a change in its physical properties. For instance, if water is extracted, the viscosity of the protoplasm must be increased. The same change in water content may result in a change in the chemical activity of the protoplasmic solution, since chemical activity, in general, depends upon the concentration of the solution involved. How it comes about is not known, but a review of the literature of experiments upon animals and plants shows that growth is very much retarded by an external solution which extracts water. Especially is the elongation of cells retarded. The only experiment dealing with the effect of external solutions upon reproduction is that of the author upon *Stigeoclonium*. Zoospores fail to be produced in strong solutions, but are produced in large numbers in weak ones.

H. G. Timberlake: 'Cell Division in *Riccia fluitans*.' Attention was called to the fact that the cells in the region of the growing point afford excellent material for the study of nuclear and cell division in the liverworts. A distinct cell plate, whose origin and development are the same as that of the spermatophytes, can be made out with very great certainty.

Howard S. Reed: 'The Ecology of a Glacial Lake.' The lake studied is the remnant of a lake which came into existence at the close of the second glacial period; at that time its extent was considerably greater than at present. As the water level slowly fell, aquatic and semi-aquatic species had the first opportunity to get a foothold and become established upon the land thus uncovered; as a result, the flora of the region shows a scarcity of distinctly terrestrial plants. The plants at the lake are grouped in five concentric zones occupying all the lake bottom less than twenty feet under water and the shores. The zones which have been named from their characteristic plants are as follows: (1) *Potamogeton*, (2) *Nuphar*, (3) *Carex* and *Sphagnum*, (4) *Salix* and *Populus*, (5) *Gramineæ* and *Compositæ*. The position of these zones is not permanent; they are steadily encroaching upon the lake and filling it with the soil they produce. The most important agencies in causing the advance of the zones into the water are soil, light and the morphology of the plants. As the plants make the lake more and more shallow they make it more unfit for themselves and fit for the succeeding zone. The struggle in each zone is less successful on

the landward than on the lakeward side of that zone. The plants engaged in this severe struggle show a marked tendency to mass themselves in solid ranks. Illustrated by lantern slides. The paper is soon to be published in full.

THIRD SESSION, WEDNESDAY, 9 A.M.

The meeting was called to order by the chairman, and without further preliminaries the reading of papers was resumed.

C. E. ALLEN: 'Spindle Formation in the Pollen Mother-Cells of *Larix*.' At an early stage in the prophases of the first nuclear division, fibrous material is present in considerable quantity in the cytoplasm, at first staining with the triple stain like the rest of the cytoplasm. Soon the fibrous material shows a tendency to stain deeply blue. It is now seen to form an irregular reticulum throughout the cytoplasm. The fibers gradually arrange themselves radially to the nucleus; the shorter ones grow in length until a complete system of radial fibers is formed, connecting the nuclear membrane with the plasma membrane. These fibers now fold over, so that many of them come to lie parallel with the nuclear membrane, and in time to form a dense felted layer immediately outside the nucleus. From the felted layer, the multipolar spindle and finally the bipolar spindle are formed, substantially as described by Belajeff and Strasburger. The most important point brought out by the investigation is that there is a fibrous system whose history can be traced from a reticulated stage to that of the completed spindle. No centrosomes could be seen, and the possibility of their presence as cell organs or directive centers seems to be excluded. The changes in the arrangement of the fibrous system seem to be correlated with processes going on within the nucleus.

BRUCE FINK: 'Some Interesting Lichen Formations.' The author made some preliminary statements regarding our present knowledge as to factors upon which ecologic studies may be based. These factors are physical and chemical structures of substrata and the structure of lichen thalli. This introduction was followed by a discussion of some of the more common lichen formations, viz., those of smooth and rough bark, those of the boulders of our prairies, and those of calcareous pebbles or horizontally disposed calcareous rocks and calcareous earth.

H. C. COWLES: 'Ecological Problems connected with Alpine Vegetation.' Alpine problems, like all ecological problems, present two aspects, phytogeographic and morphological. Most previous field studies of alpine vegetation have failed to separate distinct phytogeographic ideas. Properly to interpret alpine conditions it is necessary to distinguish floristic distribution from ecological distribution. Again, ecological distribution has its climatic and edaphic aspects. Alpine conditions have been largely regarded as climatic, and most of the peculiarities of alpine plants, distributional as well as morphological, have been referred to atmospheric factors, such as light, temperature, moisture, air. Perhaps alpine plant forms are in the main to be regarded as the direct result of external atmospheric conditions, as Bonner has shown. The distribution of alpine plants, however, is apparently due in large degree to edaphic conditions. The timber line in general may probably be referred to atmospheric conditions, but the marked gaps and oscillations which usually occur are due in a large measure to soil relations. While xerophytes increase in the alpine parts of mountains, it is to be observed that edaphic as well as climatic factors become more xerophytic upwards. While

changes occur as one traces one type of edaphic formation upwards, these changes are far less marked than are those observed in passing from one edaphic formation to another. Alpine, as well as all ecological problems, can be ultimately settled only by experimentation, and in this great field Bonnier has led the way. The field study of ecology should be regarded chiefly in the light of furnishing an intelligent basis for experiment. Illustrated by lantern slides.

R. A. Harper: 'Cell Division in Certain Blue-Green Algæ.' (No abstract furnished.)

C. R. Barnes: 'The Significance of Transpiration.' In this paper the author seeks to present a new point of view regarding transpiration, taking account of the extensive results of experimentation already attained. The purpose of transpiration is ordinarily held to be double: (*a*) to cause the influx to the leaves of a large quantity of water, that thereby a sufficient amount of mineral salts may be supplied to the leaves; (*b*) to concentrate the extremely dilute solutions thus brought to the leaves and so get rid of surplus water. These two phases of the function are held by the author to be, to some degree at least, mutually exclusive. The amount of salts absorbed is certainly dependent upon the living cortex of the rootlets and the mesophyll of the leaves. (For the purpose of the present discussion the xylem bundles may be conceived as furnishing no obstacle to water flow.) If the cortex be freely permeable, equilibrium in the distribution of any given salt will occur, assuming for a time no evaporation from the aerial parts. If then evaporation concentrates the solution the higher diffusion tension of that salt will tend to drive it to those regions where the diffusion tension is lower. This tendency, therefore, would operate against the further supply of that material to the leaves. If the cortical layers be not freely permeable, the amount absorbed is regulated wholly by protoplasmic activity and cannot be affected directly by the outside supply. The phenomena of selective absorption show that transpiration does not determine in these cases the amount of salts absorbed. The significance of transpiration is to be discovered by examining its origin and tracing its development. Under the present organization of plants exposure of wet cell walls to the atmosphere is indespensable for the solution of necessary gases, oxygen and carbon dioxid, the plant being debarred from waterproofing the cell wall so long as gas absorption is necessary. Transpiration is, therefore, considered as *unavoidable*, though in itself a constant menace to life and activity. Advantage has doubtless been taken of the xylem bundles to facilitate the movement of solutes, but there is no reason to think this essential. Transpiration also has become a protective factor with sun plants, whose temperature is thereby kept within reasonable bounds. (Since reading the paper the author has ascertained that in certain points his view of transpiration coincides with those expressed by Dr. C. E. Bessey in a paper on the function of stomata, published in Science, N. S. 7: 13–16. 1898.)

R. A. Harper: 'Binucleate Cells in Certain Hymenomycetes.' (No abstract furnished. The paper is published in full in the *Botanical Gazette* 33: 1–25. *pl. 1.* 1902.)

James B. Pollock: 'An Abnormal Development of the Prothallium of the Pollen Grain in *Picea excelsa.*' The author reported a case of a pollen grain of *Picea excelsa* in which there were four cells formed

in addition to the number usually present. These four additional cells lay in one row along the external wall of the pollen grain, between the partially disintegrated prothallial cells and the external wall, against which the first prothallial cell usually lies. The four additional cells averaged about half as large as the so-called body cell or spermatogenous cell, and the row of four was almost as long as the full width of the central portion of the pollen grain. Against the thin wall which divided the four extra cells from the large cell of the pollen grain, the cells which are usually present in the pollen grain of *Picea excelsa* were arranged in their usual manner. Two partially disintegrated prothallial cells were present, also the stalk cell and spermatogenous cell. Two interpretations are possible as to the meaning of the four extra cells: (*a*) They may show merely a spontaneous variation of the pollen grain—that is, a variation whose cause is wholly hidden in the present state of our knowledge. In this case the variation would have no special significance in the interpretation of homologies. (*b*) The four extra cells may represent a reversion to an ancestral form, and could properly be called a prothallium. If this view of the case is the correct one, all the rest of this pollen grain—that is, all that is usually present in the pollen grain—may well stand for a single antheridium, and the so-called prothallial cells are the partially disintegrated cells of the antheridium stalk. The ordinary pollen grain of *Picea excelsa* is then merely an antheridium and has no cells that may be called prothallium. In the nature of the case the proof of the latter interpretation is practically impossible, since only rarely will pollen grains be found to vary in this way. If many pollen grains should be found varying in just this same way the author would be inclined to accept the latter interpretation.

The following business was transacted:

Conway MacMillan presented the following resolution to be laid on the table until the final session: "*Resolved,* That this group hereby organize under the name of the Botanists of the Central States, and resolved, further, that the chairman be empowered to appoint a committee of three, including himself, which shall have full charge of organization, membership qualification and the program for one meeting in 1902 in case it is decided to convene during that year." After discussion the resolution was tabled for later consideration.

The secretary was asked to read a communication from W. G. Farlow, accompanying copies of the 'Third Report of the Committee on Securing Better Reviews of Botanical Literature,' which were then distributed to the botanists present. On request, William Trelease explained the progress in the organization of the International Association of Botanists and especially the plans for conducting the editorial work of the *Botanisches Centralblatt,* now the official publication of the Association. He explained also the financial plans for conducting the *Centralblatt.* It was explained that it was the plan of the *Centralblatt* to publish brief abstracts of all of the more important botanical papers, irrespective of authorship and without comment; prompt cooperation of authors and subeditors would accomplish this.

The discussion of the subject, 'Cooperation among research laboratories to avoid unnecessary duplication of work,' was opened by J. M. Coulter and participated in by R. A. Harper, William Trelease and E. E. Bogue.

In the afternoon the botanists met with the American Society of Naturalists and listened to the discussion on the relation of that Society to present and proposed scientific organizations.

FOURTH SESSION, THURSDAY, 10 A.M.

The meeting called to order by the chairman. The resolution of Conway MacMillan was taken from the table and discussed. After amendment it was adopted in this form: "*Resolved*, That this group hereby organize under the name of the Botanists of the Central States; and resolved, further, that the chairman be empowered to appoint a committee of three, including himself, which shall report to the next meeting of this body a plan of organization." The chairman accordingly appointed as such committee Conway MacMillan, D. M. Mottier and himself.

William Trelease called attention to the fact that as the American Association for the Advancement of Science and the American Society of Naturalists would meet at Washington, D. C., in January, 1903, it would be desirable for the Botanists of the Central States to convene there also at the same time. It was voted that the next meeting be held in Washington, in Convocation Week, 1903. Discussion continued as to the desirability of a general union of botanical societies to constitute a really national organization, thoroughly representative, and with autonomous local sections, *e. g.*, at present Atlantic and Central sections, and as soon as possible Pacific and Gulf sections. Such a plan of organization would combine regional convenience with national authority.

At the close of the discussion the reading of papers was continued.

CLIFTON D. HOWE: 'The Development of the Flora on a Delta Plain in Vermont.' A delta plain formed during or subsequent to the glacial period at the mouth of the Winooski river has been exposed by the gradual subsidence of Lake Champlain. The lake is now 240 feet below the general level of the delta plain. The first terrestrial flora of the plain was a sand beach flora which crossed the plain with the constantly receding beach. Then came plants which, by continually increasing the amount of humus, prepared the soil for the pitch pine (*Pinus rigida*) forest, now the controlling formation on the plain. The gentle slopes of the ravines in the now much dissected plain are controlled by a mesophytic forest of the maple-beech type. As the erosion brings the plain nearer a base level, conditions will become more and more favorable for the further extension of a mesophytic forest.

CHARLES F. HOTTES: 'Functions of the nucleolus in plants.' (No abstract furnished.)

H. N. WHITFORD: 'The Physiographic Ecology of a Sand Spit Near Cold Spring Harbor, Long Island.' (Read by title.)

J. M. WESTGATE: 'Genetic Development of the Vegetation on an Island in the Kansas River.' In this paper the author reports the results of four years' ecological study of an island in the Kansas river. The location of the island is such that the silt deposits are heavy, and as a consequence the development of the mesophytic flora from the xerophytic flora of the sandy border is rapid. Serial photographs and notes have recorded the more salient features of the changes from year to year. The succession of formations as the mesophytic conditions obtain have been largely verified by comparative studies along the Kansas and other rivers of the Mississippi basin.

All of the botanical papers announced on the printed program were read, with the exception of the one by the chairman, which he passed by. The abstract is as follows:

JOHN M. COULTER: 'Parthenogenesis in Seed Plants.' The term is used in its strict sense as meaning the segmentation of an

unfertilized egg. Two clear cases of parthenogenesis among seed plants have been published, namely, that of *Antennaria,* by Juel, in 1898, and that of certain species of *Alchemilla,* by Murbeck, in 1901. Dr. J. B. Overton, in a thesis about to be published in the *Botanical Gazette,* announces the same phenomenon in *Thalictrum purpurascens.* In this last case the segmentation of fertilized and unfertilized eggs was compared. In the former case the segmentation occurs synchronously with that of the definitive nucleus, while the unfertilized egg delays division until the very numerous free endosperm nuclei are parietally placed. It is surrounded by a very dense mass of granular cytoplasm, and associated with its segmentation are striking changes in the zone of cytoplasm immediately in contact with the egg. Overton suggests the possibility of an enzyme being secreted by the egg, and a digestion of the cytoplasm. If this be the case, substances may well be developed in the changing cytoplasm that will bring about those physical changes in the egg that induce segmentation. Observations in other species were mentioned that indicate the possibility that parthenogenesis may be a much more common phenomenon among seed plants than has been supposed. The suggestion was also made that in any embryo sac rich in cytoplasm a parthenogenetic embryo may arise.

The chairman called attention to the model herbarium and the collection of economic plant products at the Field Columbian Museum, to which the visiting botanists would be admitted free on presentation of their registration cards. In conclusion he spoke of the interest in the meetings, as evidenced by the large number who attended all of the sessions, and of the fact that this third successful meeting of the Botanists of the Central States, a body without organization, showed that its success depended upon the spontaneous interest taken in botanical work.

ALBERT SCHNEIDER,
*Secretary.*

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## SCIENTIFIC BOOKS.

*Towers and Tanks for Water Works.* By J. N. HAZELHURST, Mem. Am. Soc. C. E. New York, John Wiley & Sons. 8vo. Pp. 216; 19 illustrations.

In this work the author has evidently aimed not only to discuss those features of structural design peculiar to stand-pipe and tank construction, but also to include sufficient information relating to some of the more general matters as to make the volume complete in itself. Out of the eleven chapters of the book he thus devotes two chapters to the consideration of the properties of iron and steel, two to elementary mechanics, one to the subject of foundations, and one to the painting of steel structures. The remaining five chapters deal more specifically with the design and construction of tanks, although they also contain much of a general and elementary character.

While the engineer will find such subjects as foundations, and iron and steel, much more fully treated in special works, it is certainly convenient to have in concise form such information on these subjects as will be of direct application to this particular field of design. The chapter on painting is valuable and quite in place here, owing to the great lack of information on this important subject. The subject of riveting is quite fully treated, and convenient tables are given for the use of the designer.

In the chapters treating of the principles of mechanics and their applications to the design of the structures under consideration there is much to be criticised. This portion of the book is in fact full of the grossest errors of theory, and were it not for the very absurdity of the mistakes it would be unfortunate for such a book to come into the hands of a young engineer. The treatment of tanks is also very incomplete, no consideration being given to six- or eight-post towers and practically none

to the calculation and design of tank bottoms. We are warned, however, in the introduction not to expect 'elaborate calculations and deductions based upon problematical theories and conditions,' but only 'such facts as may have been verified, freed, as nearly as may be possible, from the tons of mathematical rubbish,' etc. The following are, presumably, some of the 'verified facts': On page 59 it is stated that 'the moment of forces about a point may hold each other and establish equilibrium of the body, even though the forces themselves fail to balance.' Also that 'the direction of the resultant of two forces is exerted in a line bisecting the original angle at which the forces met, and the extent of the force exerted by this resultant is the difference between that offered by the two or more original forces, or the moment of those forces.' Again, in Chapter VIII., in the analysis of the stresses in a four-post tower, scarcely any of the stresses have been correctly determined. The tower legs are straight and have an inclination of one in ten; the wind bracing is of the usual type, consisting of horizontal struts and diagonal tie rods. The method of calculating the compression in the struts is as follows: "The inclination of the column being one in ten, one-tenth of the load is transferred to the horizontal member as compression-stress, and the remaining nine-tenths is distributed at the base of the column to the foundation." The column stress being 133.9 tons, the thrust against the strut is therefore 13.39 tons; but, since the thrust from each of the two opposite columns is 13.39 tons, the strut must be designed to resist *twice that or 26.78 tons!* The stress in the strut 'in transferring the wind stress as tensile stress' is not considered, this member being designed only for the compression as above found, together with the stresses due to its own weight. In finding the wind stresses in the diagonals of the upper panel, the stress in each is taken at one-eighth of the total wind pressure on the tank, presumably because there are eight diagonals in the top story of the tower. In this way the stress is computed to be about eight tons, with an assumed wind pressure of seventy tons, whereas the correct stress is about thirty-two tons. Finally the wind stress in each column is taken as constant from top to bottom.

These and other illustrations which could be given suggest that it might have been better to admit some of the 'mathematical rubbish' so carefully excluded.

F. E. T.

*Geometric Exercises in Paper Folding.* By T. Sundara Row. Edited and revised by Professors W. W. Beman and D. E. Smith. Published by the Open Court Publishing Company, Chicago. 1901. Pp. x+148.

In the author's preface to this little work, dated from Madras, India, 1893, the double purpose is set forth 'not only to aid the teaching of geometry in schools and colleges but also to afford mathematical recreation to young and old, in an attractive and cheap form.' Without attempting to develop a geometry as rigidly confined to folding as the Euclidean is to compass-and-ruler work, it is shown how a large number of interesting metrical and positional relations can be illustrated without the use of instruments other than a penknife and scraps of paper, the latter for setting off equal lengths on folds. Sheets of paper adapted to the work accompany the book, and the allusions in the text to certain kindergarten 'gifts' imply the pupil's possession of an equipment of elementary geometric forms. The processes are based on the principle of congruence.

The first nine chapters are devoted to the regular polygons of Euclid's first four books, and to the nonagon. Beginning with the folding of the fundamental square, and progressing through equilateral and other triangles, the Pythagorean theorem and consequent propositions are reached, with certain puzzle squares based thereon. In Chapter X. progressions—arithmetic, geometric and harmonic—are neatly illustrated, as also the summation of certain series. This section is enlivened by the insertion of the legend regarding the duplication of the cube. It would have been an appropriate place to refer to the adaptation of the cissoid and conchoid of Chapter XIV. to the same problem.

In Chapter XI. the numerical value of $\pi$

is calculated and the regular polygons treated, in particular those of five and of seventeen sides.

Congruence, symmetry, similarity, concurrence and collinearity are taken up in the next section, and Desargues's, Pascal's, Poncelet's and other famous theorems presented for demonstration.

The remaining chapters treat of conics and other plane curves, with historical notes and references to certain applications, completing in an attractive way a valuable addition to the literature of elementary geometry—a serviceable condensation of mathematical properties, theorems, puzzles and problems. We may be permitted to doubt, however, whether the average student who has attained to that acquaintance with radicals, logarithms and positional geometry which is evidently assumed in Chapters XI.–XIV., will often stop to obtain his actual results by folding. In fact the frequent use of the word 'draw' implies the author's permission of a short-cut; but it would probably be an encouragement to the pupil actually to bring his folding into the higher problems if in connection with it the use of the compass, dividers and straight-edge were frankly sanctioned. Simply in the interest of accuracy in folding, a thin rule, preferably of nickel-plated steel, beveled, would be desirable.

Where the claim of the author is so modest and his aim in so high degree attained, the task of criticism is a light one. It is singular that the expression 'equal halves,' if in the original, should have passed two revisers unnoticed; and one could wish that pericycloids, the involute and the cartesian ovals had not been omitted, and that the relative importance of the curves treated were better indicated by the space allotted to them.

The editors have performed a genuine service in bringing this work before an American audience and in such neat and attractive form. The twenty-six exquisite half-tone illustrations with which they have replaced the line drawings of the original, are a decided enrichment of the volume. The practically equal number of footnote references to their own series, in one case duplicated, compels the question how far permission to edit carries with it advertising privileges.

F. N. WILLSON.

PRINCETON, N. J.,
February, 1902.

*Pleuronectes (the Plaice).* By F. J. COLE and JAMES JOHNSTONE. Liverpool Marine Biology Committee Memoirs, No. 8. London, Williams & Norgate. Dec., 1901. Pp. 260, 11 plates. Price, 7s.

In these L. M. B. C. Memoirs a single animal or plant type is described by a specialist in such a way as to serve primarily the interests of college and private students of biology and young amateurs. They are, however, far more than mere laboratory guides, being authoritative sources of information based on original work upon species which for the most part are not elsewhere adequately described.

This, the latest memoir of the series, is devoted to an important food fish, the plaice, containing descriptions with excellent figures of the skeleton, abdominal viscera, blood vascular system, nervous system and sense organs, together with appendixes on life history, habits and practical fishery matters. Its chief interest for biologists in general lies in the discussion of the asymmetry of the Heterosomata, or flat fishes, of which the plaice is probably the best known British representative.

In explaining this asymmetry the authors follow Traquair, disposing first of the mischievous assumptions that the left eye has passed either through the substance of the head or over the top of the head to reach its definitive position on the right side of the body. "The *fact* is," they remark, "that the left eye is *not on the right side at all.* Its presence there is purely illusory. What has happened is that the *whole* of the cranium *in the region of the orbit* has rotated on its longitudinal axis to the right side, until the two eyes, instead of occupying a horizontal plane, have assumed a vertical one, and the left eye is *dorsal* to the right."

The part of the work next in importance to the discussion of the asymmetry is the section devoted to the cranial nerves, which are given a thorough critical treatment. The key to the

comprehension of the cranial nerves is the doctrine of nerve components as developed (chiefly by American students) during the past decade, a doctrine which apparently very few neurologists in Europe have yet really comprehended. The fifty pages of this work devoted to the peripheral nervous system will serve as an admirable and not too technical introduction to this important subject, and will doubtless hasten the day when it will filter down into the text-books.

C. Judson Herrick.

## *SOCIETIES AND ACADEMIES.*

### RESEARCH CLUB OF THE UNIVERSITY OF MICHIGAN.

Since last reported this Club has held two meetings, one on December 18, 1901, the other on January 8, 1902.

At the former meeting, Dr. A. R. Cushny read a paper on 'Renal Secretion and Diuresis,' in which he first discussed the two chief theories on the subject and then attempted to apply them to the explanation of the diuresis induced by the intravenous injection of saline solutions. When a mixture of sulphate and chloride of sodium in equal parts is injected, the chloride of the urine first exceeds the sulphate in amount, while later the reverse is the case. This is most simply explained by the reabsorption of chloride in the renal tubules, which take up this salt much more readily than the sulphate. When the absorption is accelerated by partial closure of the ureter, which increases the pressure in the tubules, the chloride of the urine diminishes much more than the sulphate. The behavior of the chloride and sulphate of the urine thus confirms Ludwig's theory that the renal tubules are absorptive rather than secretory organs. In the discussion which followed, it was intimated by the reader of the paper that there were grounds to believe that the secretory cells of the renal capsule are unable to discriminate between sulphate and chloride and that the relative amounts of these in the glomerular fluid is determined by their relative proportion in the plasma of the blood.

At the conclusion of Dr. Cushny's paper, Professor Henry C. Adams spoke on 'Trusts.' Giving at first the older classification of business and commercial organizations as limited by profitable administration, the speaker devoted his time to the enquiry as to whether conditions have so changed as to make possible the profitable combination into one organization of two or more formerly economically distinct classes of business.

At the meeting of January 8, Dr. Guthe spoke on the action of the coherer with special reference to the investigations which he has published in the *Annalen der Physik*, 4, p. 762, 1901, and in the *Physical Review*, 12, p. 245, 1901.

After a short description of the single contact coherer used by him and an explanation of the so-called decohesion, he calculated how near the metallic surfaces must be brought together in order to produce coherer action. The work of Earhart on sparking distances leads to the conclusion that the insulating layer can only have a thickness of a fraction of the wave-length of sodium light, while the distance corresponding to the critical voltages of different metals, as found by him, must be of molecular dimensions. Thus the thickness of the air film, if the original high resistance is really due to such a film, can be only a very small fraction of its normal value. But it seems unnecessary to assume the presence of a layer of air between the surfaces in all cases in which coherence takes place. The decrease in resistance or actual metallic contact between the coherer particles, Dr. Guthe believed to be due mainly to the welding together of the metals at the point of contact by the heat produced when even a minute quantity of electricity passes through an extremely small area of high resistance.

Dr. Guthe was followed by Dr. S. J. Holmes, who spoke on 'The Habits of Amphipods,' detailing many interesting actions in their life history. Portions of the results obtained by Dr. Holmes have been published in the *Biological Bulletin* and in the *American Journal of Physiology*. The later observations have appeared in abstract in Science in the report of the Chicago meeting of the Morphological Society.

Frederick C. Newcombe,
*Secretary.*

ZOOLOGICAL CLUB, UNIVERSITY OF CHICAGO.

MEETING OF NOV. 20, 1901.

'Experiments in Grafting *Hydra*': MARY HEFFERAN.

These experiments were carried on during the year 1900 at the University of Chicago, and were based upon the similar work of Rand (1899) and Miss Peebles (1900). A comparison of the behavior of lateral grafts in the two species *Hydra fusca* and *Hydra viridis* showed a marked difference in the process of regulation. In the former, the graft moved up the stock until the head ends of stock and graft were of the same length, forming a Y-shaped figure. Then the two trunks gradually fused into one. A graft inserted very low down on the stock, *i. e.*, in the aboral 1/5, might constrict off from the foot. In *Hydra viridis* the process was quite the contrary. The graft moved down the stock instead of up, and finally separated from it at the foot instead of fusing as in *Hydra fusca*. The difference in size of the two species and the action of capillarity is suggested as an explanation of these different processes. In tangent grafts fusion took place the more readily as the area of union was increased in grafting. When poles were reversed separation took place if the area of union was so large that the polyps were unable to twist around in order that fusion could follow with poles in the same direction. It was impossible to build up *Hydra* of abnormal length by grafting several polyps together end to end. Normal form was regained usually by constriction and separation at the point of grafting, or when the compound was not much more than the ordinary length, by gradual reduction through absorption. In a few cases buds formed on such compounds soon after grafting. These buds arose entirely out of the budding region of the individual components, but within what would be the budding zone of the whole. The general results may be summed up in the words of Wetzel, '95: 'Ueberall zeigt sich ein deutliches Streben, die normal Gestalt wieder herzustellen.'

MEETING OF DEC. 4, 1901.

'Some Observations upon the Eye of *Bdellostoma Stouti*': B. M. ALLEN.

The eyes of this Pacific coast myxinoid show a very primitive structure, which is in reality the result of a complex process of degeneration. The eyeball is found imbedded in a mass of fat about three times its size. In one case, the eye was found to lie some distance beneath the outer surface of the mass of fat. Normally, however, the corneal surface lies on a level with the surface of the fat and is often flattened to form a rather extensive free surface. No eye muscles nor traces of such were discovered. No oculomotor nerves were found. No traces of them are discoverable in embryonic life (Kupffer). There is no trace of a crystalline lens. According to G. C. Price and Kupffer, a rudiment of a lens occurs at a very early stage of embryonic life, but very soon disappears. The choroid and sclerotic coats are represented by a very thin layer of unpigmented, non-vascular connective tissue without any appreciable distinction between corneal and sclerotic portions. The retina remains in the early condition of an optic cup, the outer layer (pigment layer) not being fused with the remaining layers. All specimens showed the layer in question to be widely separated from the bulk of the retina. This pigment layer is composed of a single layer of cubical cells devoid of pigment as far as I could ascertain. A layer corresponding to that of the rods and cones in higher vertebrates is clearly present. The nuclei of these structures (outer nuclear layer) are strikingly well developed and regularly arranged. Certain characteristic cells of the inner nuclear layer could be readily made out. It is impossible at present to give an accurate account of the minute histological details of this or of any other part of the retina, owing to the lack of living material. The ganglionic layer is represented by cells scattered irregularly throughout the inner reticular layer. Fibers from these last named cells can be traced in a more or less direct course to the optic nerve. The outer rim of the optic cup is in many cases differentiated in such a manner as to suggest a rudimentary iris. A structure unmistakably like an iris was found in one specimen examined. The cellular structure of this rudimentary iris is almost identical with

that of the pigment layer. No indications of muscle fibers or pigment are to be seen. Certain deeply staining coagula within the optic cup give evidence of a vitreous body. Some large, clearly marked cells, probably those of the vitreous body, are found attached to the surface of the retina. Evidences of a choroid fissure are to be seen in the fact that the ventral portion of the retina is thinner than the dorsal in almost all specimens. In one case the choroid fissure was found to persist. The most striking feature, however, is the extreme variation. The optic nerve enters the eye at various angles. Variation occurs in all parts of the eye, and is especially notable in the measurements of the thickness of the retina and the dimensions of the eye as a whole.

C. M. Child,
*Secretary.*

BIOLOGICAL SOCIETY OF WASHINGTON.

The 350th regular meeting was held on Saturday evening, February 22.

C. H. Townsend spoke on 'The Present Status of the Carp in American Waters,' saying that in spite of much adverse comment this fish was rapidly assuming an important place in this country and that no less than $400,000 worth was sold annually, largely in New York. It was the source of the principal fishery in the Illinois River where the bass had increased in spite of statements that carp destroyed the spawn and young of bass. The speaker believed that when the proper methods of raising and cooking carp were better appreciated it would find much favor and be an important article of food, especially among those who could not afford the prices for the most desirable species. It would be impossible to propagate the finer species of fish on a sufficient scale to keep pace with our growing population and as the carp could be readily raised it would supply the deficiency caused by the lack of other fishes.

C. P. Hartley presented a paper on 'The Pollenation of Immature Flowers,' saying that, in order to save labor, plant breeders sometimes apply pollen to flowers at the time they emasculate them. Because fair success has often resulted from this method it is now quite universally taken for granted that pollen placed on immature pistils will remain there until the pistils are receptive and then fertilize the flowers. Experiments with tobacco prove that there are flowers that are killed and caused to fall from the plants by being pollenated before their pistils are mature; and microscopic study of flowers so treated shows that the pollen germinates on the stigmas sending pollen tubes down the immature pistils into the ovaries. This growth of pollen tubes in the ovaries among ovules not sufficiently mature to admit of fertilization causes the flowers to fall. Tobacco flowers fall in about thirty-six hours after being prematurely pollenated. If pollenated when almost mature, *i. e.*, eighteen or twenty-four hours before the flowers would have opened, many will set fruit; but if pollenated two, three or even four days before maturity, the flowers invariably fall, separating smoothly from the plant at the base of the peduncles.

*Datura* flowers are also killed by premature pollenation, though unlike tobacco flowers they do not fall but wither away and fail to develop seeds. Doubtless other kinds of flowers will be found to be injured by premature pollenation. The growth of the pistils of cotton blossoms is checked by premature pollenation and flowers pollenated one day before maturity do not set so many nor produce as good fruits as those pollenated at maturity. Tomato blossoms fail to set fruit when pollenated six days before maturity, the failure being due to loss of vitality in the pollen. If the flowers on becoming mature be again pollenated they set fruits. Orange blossoms pollenated nine days before maturity are not injured but continue their growth and mature good fruits. This is true of seedy as well as of navel oranges and the fact that flowers of the navel oranges so treated result in fruits containing good seeds, proves that the pollen so early placed on the stigmas successfully fertilizes the flowers.

The experiments show that certain kinds of flowers are killed by being pollenated too young; other kinds fail to set fruit because the pollen placed on the young stigma loses its vitality before the pistil becomes receptive,

while still other kinds will set fruits although pollenated while quite immature.

Lyster H. Dewey discussed 'The Identity of Prickly Lettuce,' stating that a plant bearing this common name, and generally considered to be *Lactuca scariola,* was introduced into the United States in the early sixties and spread with such rapidity as to become the most widely distributed exotic weed. During the summer of 1901 specimens of true *L. scariola* with runcinate leaves were received from Hamilton Co., Ohio, and this led to a reexamination of the species. It was at first thought that a common form of the American plant having leaves merely spinulose-margined, but entire or slightly wavy in outline, was *L. virosa* L. This European species however has rather large, oblong-obovate, thin leaves, not twisted to a vertical plane as are the rather thick, firm leaves of our prickly lettuce and further study proved our form to be *L. scariola integrata* Gren. et Gord. A few specimens examined exhibit a gradation between this variety and the typical form.

F. A. Lucas described 'The Armor of *Stegosaurus,*' saying that this consisted of large plates standing on edge on the back and several large spines on the tail. The first Stegosaur, *Omosaurus,* was found in England, and Professor Owen considered that the tail spine belonged on the wrist. The broad dorsal plates found with the first American specimen, belonging to the genus *Stegosaurus,* were thought to have been imbedded in the skin like the much smaller plates of the turtle *Sphargis.* It was soon recognized however that they belonged on the back and the animal was restored with a line of plates down the center of the back. Subsequent study showed clearly that there were two rows of plates, one on either side of the median line, and probably but two pairs of spines on the tail. The most recent comparisons seemed to indicate that the large upright plates were not disposed in pairs, but had an alternating arrangement, although this was unlike the armature or adornment of any other known animal.

F. A. LUCAS.

## NEW YORK ACADEMY OF SCIENCES.

### SECTION OF GEOLOGY.

THE regular meeting of the Section was held on January 20, with a comparatively large number of members present, and the following program was presented:

Professor R. P. Whitfield read two papers. The first was upon the Ammonite *Heteroceras simplicostatum,* in which he emended and elaborated the description of that species which he had given in the Newton and Jenny Report on the Black Hills, published in 1880, the new observations being based upon material gathered by Dr. E. O. Hovey on an expedition of the American Museum last summer. This material shows conclusively that the three genera *Hamites, Ancyloceras* and *Heteroceras* have no independent existence, because single individuals show the distinguishing characters of all three genera combined. This fact had been suspected by the author when at work upon the Newton material twenty-five years ago, and it has been hinted at in the writings of Hyatt and others, but these were the first specimens described which settled the question.

Professor Whitfield's second paper described a new teredo-like shell from the Laramie group of eastern Wyoming, collected by Mr. Barnum Brown, of the American Museum. This teredo, to which the author has given the name *Xylophomya laramiensis,* is more than an inch in diameter, thus ranking with the largest species of the family known.

These two papers may be found in full in the current volume of the *Bulletin of the American Museum of Natural History.*

The third paper of the evening was by Professor James Douglas and gave a description, illustrated by a topographic map and numerous lantern slides, of the famous Rio Tinto group of the copper mines of the Huelva district in Spain. These mines have been worked from time immemorial, the earliest knowledge of them dating from the Phœnicians, who occupied the country in the eleventh century, B.C. The Romans also obtained a large amount of copper from these deposits, and it is an interesting fact that the slags which they left are purer—that is, freer from copper, than

those which are made there to-day. The ore is a copper-bearing pyrite, carrying some silica. The copper-bearing portions run irregularly through the iron pyrites, and the Rio Tinto Company has removed millions of tons of forty-two per cent. iron ore in getting at its copper ore. The iron ore is not profitable at the present time, although it may become so in the distant future. There are some remains of the workings of the ancients here. At Tharsis, in particular, the old shafts are very peculiarly constructed, one at least being spiral to enable the miners to carry the ore on their backs. Shelves were excavated at intervals in the walls of the shaft to enable the men to rest their loads on their weary journey to the surface.

The mines are worked now as open air diggings in circular terraces. They produce about two million tons of ore per year, and it is estimated that there are one hundred and sixty million tons in sight. Some silver-bearing galena is associated with the copper ore. The old-fashioned method of roasting the ore in heaps was kept up until 1893, but the ore is now leached by means of water. This is a long process, requiring four years for its thorough completion, but the copper is leached out so that less than one-fourth of one per cent. is left in the tailings. The great bulk of the world's supply of sulphuric acid is obtained from the Rio Tinto pyrite, which is shipped all over the world for the purpose of manufacturing the acid. Five hundred thousand tons per year are utilized in this way.

The paper was discussed by Dr. Julien and Mr. Howe, and the Section passed a hearty vote of thanks to Professor Douglas for his kindness in giving the paper.

A REGULAR meeting of the Society was held on February 17, with the Chairman, Dr. A. A. Julien, presiding.

The first paper to be read was by Dr. O. P. Hay, on the 'Snout-fishes of Kansas.' In this paper the author presented a brief history of our knowledge of the genus *Protosphyræna,* and a statement showing what portions of the skeleton were still unknown. Those parts which are best known are the skull, especially the elongated snout, and the jaws, the shoulder and the caudal and pectoral fins. These parts have seldom been found associated, and there have been established three series of species—one on the teeth, one on the snout and the third on the fins. It is certain that, as new collections are made and studied, some of these subspecies will be reduced to synonomy. The author pointed out various errors on the part of writers in the interpretation of different elements of the skeleton, and illustrated his points by means of specimens.

Dr. A. A. Julien gave an impromptu discussion of the relation of honestones to the cutting edge of tools, in the course of which he said that the quality of a hone depended on the size and shape of its component particles, and upon the cement joining the whole together, except in the case of the novaculites from Arkansas, in which the honing quality is due to the sharp edges of minute cavities left by the solution of calcite; and in the case of the Turkey-stone, in which the honing quality is due to veinlets of quartz intersecting a rock which has been formed by silica replacing a granular limestone. A microscopic study shows that the edge of a tool is not regularly serrated, part of it being smooth and part undulatory. Viewed on edge, the sharpest tools are practically straight, while the others are more or less regularly wavy. Viewed in the cross section, a fine edge is seen to be a perfect wedge, while the duller tools show a minute shoulder.

EDMUND O. HOVEY,
*Secretary.*

SECTION OF BIOLOGY.

AT a regular meeting of the Section, held on February 10, Professor W. B. Scott, of Princeton University, presented an illustrated lecture entitled, 'The Origin and Development of South American Mammals.'

The speaker began by expressing his great obligation to Dr. F. Ameghino, as also to Dr. Moreno, director, and to the curators of the La Plata Museum, for their kindness in giving him the freest use of their collections and enabling him to examine all the types of the Santa Cruz mammals.

The fauna of every continent is made up of

two elements, the indigenous forms which were developed in that continent, and the immigrants from other regions. In South America this distinction is easy to draw, because of the remarkable series of Tertiary deposits which are wonderfully rich in well-preserved fossils. The Santa Cruz beds, which are almost certainly referable to the lower Miocene, contain an assemblage of mammals altogether different from those of the northern hemisphere. The fauna consists of Primates and Insectivora, very scantily represented, very numerous Rodents (though all referable to the Hystricomorphs), Marsupials, Edentates and the peculiar South American hoofed animals. The Edentates of this period represent the Gravigrada, Glyptodonts and Armadillos, but no members of the true Sloths or Anteaters have yet been found, a lack of which is probably due to climatic conditions. The Gravigrada, which are very abundant, have forerunners of all the great Pleistocene groups, but are, of course, much less specialized and are relatively small in size. The Glyptodonts, though numerous and well preserved, are not so easily to be brought into relation with the later genera of the same group.

The paper concluded with a brief examination of the remarkable Ungulates, all of which are peculiar to South America, and especial attention was called to Ameghino's discovery, yet unpublished, that in *Nesodon* there are three sets of functional incisors and canines. Incredible as such an observation may be, it seems to be well established.

HENRY E. CRAMPTON,
*Secretary.*

## THE BOSTON SOCIETY OF NATURAL HISTORY.

AT a meeting of the Society held January 1, 1902, Dr. George H. Parker gave an account of some experiments which he had conducted on the marine Copepod, *Labidocera æstiva,* with a view to accounting for the fact that it is extremely abundant on the surface of the water along shore at night, but during the hours of daylight is found only down in the deeper waters. After giving a short account of the external structure and method of locomotion, the speaker described a series of experiments with these copepods in aquaria, from which it appeared that the females are negatively geotactic, their tendency being to swim against rather than with the force of gravity. They were also found to be attracted by a light of small intensity, but repelled by a brilliant illumination. The reactions to light seem to be stronger than those to gravity. The diurnal migration on the part of the females is thus to be explained as being due to their endeavor to seek a region of such depth below the surface of the water as shall have the requisite intensity of light. The males of this species seemed to show no very definite response to light or gravity, though their reactions indicated that they were to a slight degree negatively phototactic, and positively geotactic. By experiments with females enclosed in small glass tubes, which were covered with filter paper and plugged at the ends with cotton, it seemed evident that the females give out some sort of scent which becomes disseminated throughout the immediately surrounding water, and is strongly attractive to the males. The males, then, perform the same diurnal migration as the females, because they are attracted by the scent of the latter, and so follow in their wake. Mr. C. J. Maynard then gave an account of the habits and structure of the Anhinga and the Courlan, two Florida birds. Among the specimens shown was a preparation of the peculiar convolution of the trachea in the adult male of the latter species, a striking secondary sexual character.

At the meeting of January 15, 1902, Mr. William L. W. Field gave an account of a 'Glacial Lake Problem in Southern Vermont.' The region studied covers a portion of the basins of the Black and the Williams rivers, tributaries of the Connecticut. At a certain locality the courses of these two rivers approximate rather closely, and at this region there are two passes connecting the respective river basins, the one very narrow, with steep sides, locally known as Proctorsville Gulf, the other, farther down the valley, much broader and apparently widened to a considerable extent by ice action. From a study of the sedi-

ments and the topographic features, it seemed probable that during the recession of the glacial ice-sheet a lake had been formed, which, as the ice melted out, had discharged first through the upper pass, and later through the lower one. A number of lantern slides were shown in illustration of the topographic features of the area under discussion.

GLOVER M. ALLEN,
*Secretary.*

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## *DISCUSSION AND CORRESPONDENCE.*

### THE ENDOWMENT OF RESEARCH.

TO THE EDITOR OF SCIENCE: I have been much impressed by the communication of Mr. H. H. Clayton in your recent issue, in relation to the subject of grants for scientific research, for the reason that his views coincide so closely with mine, based on both theoretical considerations and practical experience.

On two occasions I have been the recipient of such grants, and I confess that on each occasion I labored under a feeling of constant uneasiness for fear that I might not be able to accomplish what others might consider adequate returns for the amount of the grant. This feeling may have no reason for existence and perhaps it does injustice to those who have such funds in charge, but that it exists and that it has a distinct influence upon many applicants can not be questioned. It may perhaps be objected that such persons should not, or at least that they need not, seek to avail themselves of such opportunities, but this, it seems to me, would merely result in debarring many conscientous workers, while at the same time encouraging others not so sensitive.

In regard to the effect of prohibiting the payment of personal expenses out of research funds I may not be considered a competent witness, for the reason that in the two instances mentioned I was not restricted as to the manner in which the grants should be expended and it was never necessary for me to try to draw a hard and fast line between what might be considered purely personal expenses and those which were incurred solely in connection with the actual research work. Had such restrictions been imposed, however, I believe that I should have hesitated to accept the first grant and know that I should have declined the second, on account of my inability to satisfy myself that I could draw a line so that items on either side could not be questioned or criticized.

In common, as I have reason to believe, with nearly every active scientific worker, I have always had sufficient work under way, or definitely planned, to occupy all my time for months and sometimes for years ahead, and tardiness in completing investigations has more often been due to the element of personal expenses than to any other cause. Such a condition is particularly in evidence where investigations involve the necessity of traveling. Good results can hardly be expected if the investigator is constantly harassed by having to consider whether each item of expense may be conscientiously charged to his research fund or not. The success or failure of an investigation in the field may often depend entirely upon the length of time which can be given to it, or, what is the same thing, to the sum available merely for living expenses.

In regard to laboratory work I can not speak from experience, but I do not see why any different principle should prevail in that connection than in any other. The proper basis for a grant, it seems to me, should be absolute confidence in the recipient, giving him to understand that the amount of the grant was his, to apply in any way which he might think would best accomplish, or assist in accomplishing, the object of his investigations.

ARTHUR HOLLICK.

### SCIENTIFIC NOMENCLATURE.

A PRIME characteristic of the scientific mind is the ability to enter into details and to make distinctions, as well as to see the relation between the elements of knowledge. In order that some conception of these distinctions may be communicated to another mind, names must be given to a perpetually increasing list of objects and qualities, with divisions and subdivisions. In natural science, to try to stretch an existing vocabulary and make it cover new conceptions by using old names with new

meanings, is to invite obscurity and misunderstanding.

The unscientific mind may not always appreciate the requirements of classification as an important aid to scientific development. To one who is not a geologist nor an agriculturist, a clod of earth may be sufficiently described by a word of three letters. It is mud, and there is nothing more to be said about it. But the man who has learned to use his eyes (and one need not have a college education to do that) perceives that there may be fifty different kinds of mud; and the scientist who wishes to investigate the subject of soils and the rocks from which they are made, recognizes the necessity of an exact and elaborate nomenclature.

This need comes, in the first place, from the use of terms *as mere tools for facilitating analysis,* and thus favoring the development of a research. In this sense, that is to say, as provisory terms, invented by the investigator for the purpose of mapping out and arranging his work in an orderly way, it is desirable that the vocabulary shall be so full that it may seldom or never be necessary to use names with a double significance. Not all of these names will be retained eventually, but the looker-on must learn to tolerate them, at least during the incipient stage of path-finding investigation.

In the next place, entirely new branches of knowledge require the invention of whole classes of terms, constituting virtually a new language. To dissent from this position, and to require that the new thoughts shall be clothed in familiar forms, is as unreasonable as to require that the proposition of the maximum economy of material in the construction of the bee's cell shall be demonstrated without the use of the differential calculus, or that all psychological propositions shall be stated in terms of one sense, that of sight.

The final forms which shall be given to words expressing necessary and permanently useful distinctions of meaning are a matter which may well concern all scientific workers, whatever their specialties, as well as the general public. It is of course desirable that a new word shall be short, if this desideratum is compatible with intelligibility. Unfortunately, most of the short-cuts which are proposed from time to time, such as sweeping reforms of an extensive and tremendously cumbersome chemical nomenclature by substituting words of one syllable, break down under a weight of meaningless memorizing which is absolutely prohibitive. Common names of plants and animals become overloaded with so many meanings in different localities as to be equally useless. The prevalent custom of inventing names by joining Greek or Latin words of cognate import, giving to the new term a special and new significance, has the advantage that the word-coinage is, to a degree, self-explanatory, at least to one who has learned a modicum of Greek and Latin words. There is no royal road to knowledge. Scientific descriptions remain unintelligible to the lazy man who hates to use the dictionary. They are free property to all who are willing to take this trouble.

FRANK W. VERY.

## *ENGINEERING NOTES.*

### INDUSTRIAL ECONOMICS.

AN interesting and probably important fact, and one which may ultimately have a serious influence upon the relative standing, industrially, of the United States and Great Britain, is reported by English papers. It is the signature of an agreement between the employers and workmen in the machine shops of Great Britain which, on the whole, would seem entirely reasonable, while in the United States the unions have refused to enter into a similarly reasonable arrangement. The initiation of the displacement of British manufacturers from their own markets and from the markets of the world was largely due to the restriction of production and the deprivation of free workmen of the privilege of working at their trades, while, in our own country, restriction of production was almost unknown and freedom of the individual was at least not absolutely destroyed. It now looks possible that the conditions may be reversed.

The British agreement provides that the unions shall not interfere with business management, nor the employers with the proper

functions of the unions; the men may join the unions or remain free as they may choose and the employer may employ union or non-union men; piecework is approved and restriction of output specifically disapproved. No limitation of the number of apprentices is permitted. In case of disagreement regarding any question arising between the two parties, reference and arbitration will be prescribed and work shall not stop when such question arises or during the session of the committees of arbitration.

Had these principles been in force in recent years, it is hardly to be believed that the long and costly strike which finally broke up the former tyranny would have occurred or that Great Britain would have experienced, as now, competition of serious character within her own boundaries.

On the other hand, should the false principles formerly so destructive of British industries find extensive lodgment in the United States, as now seems possible, it can hardly be doubted that the experience of the older country will be repeated in our own. Restriction of production has been a cardinal principle with many associations though, fortunately, not with the most intelligent and well-managed, nor so generally and effectively as to as yet seriously impair the industrial prosperity of the nation. The future of our industrial organization may be found to depend, nevertheless, upon the intelligence, the courage and the firmness of the leaders in the unions and upon their success in the maintenance of right principles in fixing the relations of employer and employee. Freedom in bargaining, independence of the individual who chooses to be free and independent, freedom of the ambitious and industrious and skilful, within or without the union, to secure the full value of his best efforts, and entire freedom to secure maximum output in both quantity and quality are now assured the British workman, for the first time in at least two generations, and, in default of similar freedom and independence and of similar economic practice in the United States, the tables may once more be turned. The spirit of fairness and the intelligence and knowledge of economical principles displayed by the leaders of the unions of most intelligent and highly skilled workmen in the United States and the rapidity with which a good example makes its impression in this country give assurance that the progress of the country industrially is not likely to be suddenly or soon checked. When an enormous organization like, for example, the Railway Trainmen's Association, makes fair play and industrial peace a cardinal doctrine, and when their associates of the Locomotive Engineer's unions have a record of not more than two or three serious strikes in a generation, it may be fairly anticipated that reason and justice will ultimately prevail generally.

## MR. MARCONI'S ACHIEVEMENT.

The month of February and particularly the 23d and 25th of February, 1902, will undoubtedly become historically recorded as the beginning of what may be known as the Marconian era. It was on the first of these dates that a message was transmitted more than a thousand miles, between a station on the coast of Cornwall and a ship at sea in the midst of the Atlantic, and it was at the second of these dates that distinct signals were repeatedly transmitted over a distance exceeding two thousand miles under similar circumstances and permanently recorded on the tape of the receiving instrument. The practicability of the system of wireless telegraphy operated by Mr. Marconi was thus confirmed as effectively for these enormous distances as it had been, long before, by constant use over shorter ranges, for months together, on the coasts of England, France and the United States.

The Marconi station at Poldhu, Cornwall, has been in use a long time, not simply for the usual work of exchanging messages with ships at sea in that neighborhood, but also in the investigation of the problem of transmission over the ocean, from shore to shore. Weeks before it had been found possible to reach the coast of Newfoundland with distinct signals and Mr. Marconi, returning to England, refitted his apparatus for a test which should be crucial. He left Southamp-

ton on the U. S. M. S. *Philadelphia* February 22d and, with a prearranged system, communicated with his operator at Poldhu, regularly, from a point 250 miles west of the Lizard until reaching mid-ocean, over a thousand miles away, the operator reported "Fine here. Thanks for message!" Thence, to a point 1,551 miles away, messages continued to be intelligible, the last, 'All in order,' indicating that the cessation was due to lack of power in the sending apparatus, not to any defect of construction or adjustment. Single signals nevertheless continued to be recognizable, and were automatically recorded on the tape, until the two operators were separated by 2,099 statute miles. The records of all these messages and signals were properly certified to by the operators and by the officers of the ship, in order that the scepticism manifested at the first announcement of Mr. Marconi's work in Newfoundland might not be given a shadow of an excuse for expression in this instance. During this experiment the messages and signals transmitted to the *Philadelphia* passed over the *Umbria*, following in her wake all the way across the Atlantic, or within easy communicating distance, without being recognized or even detected.

Mr. Marconi is now confident that he has demonstrated that the distance over which his method will prove available is only limited by the power of the sending apparatus. He is preparing to establish at Poldhu ten times as much transmitting power as was available on this occasion. It may probably be admitted as demonstrated that we may anticipate the successful transmission of messages between a ship at sea and the shore, on either hand, from the moment of her setting out on her voyage until her passengers are landed at her destination on the other side of the ocean. Then the previously unavoidable period of anxiety attending the disappearance of ship and crew and passengers, for days together, will be at an end forever. New, or temporary, or moving stations may be established at sea or on land, and a campaign may be conducted, in time of war, with perfect communication between forces and commanders however relatively situated and, with suitable codes, without enlightening the enemy, even if the fact of communication be detected by him at all.

R. H. THURSTON.

## *ANNUAL REPORT OF THE CONCILIUM BIBLIOGRAPHICUM.*

THE general statement for 1901 has just been issued from Zürich and shows that Dr. Field's determination to carry this project through is at last beginning to meet with reward. The total number of cards published in 1896 was 3,345, and in 1901, 21,946. The total number of cards issued up to December 31, 1901, is 9,671,500. The total expenditure up to the same date is 119,015 francs, or in round numbers $23,803. The receipts up to the same date have been 92,484 francs, thus leaving outstanding amounts of upwards of 21,000 francs or something over $4,000, probably due to losses in the two first years of inauguration, which will soon be covered by the present increasing sales.

The financial standing of the present year shows a great advance over all that have preceded; the increase of subscribers has been so great that whole sets have gone out of print. The prices charged for subscriptions correspond, however, so closely to the actual cost that the increased sales have occasioned increased expenditures to nearly the same amount. It is the generosity of the Swiss Government to which in the main the Concilium owes the present improved state of its finances. While this shows the permanence of the work, it is very desirable that other countries should give similar aid and thus remove the last of the difficulties under which Dr. Field and his staff are struggling.

In consequence of the failure of an expected subsidy, the physiological part of the work has been temporarily suspended, but it is hoped that this impediment will soon be removed.

A recent report of the Swiss Society of Naturalists estimates the saving of time afforded by the great catalogue in the specific case of an investigation on the trout; the report says that in looking up the recent literature of this subject by means of the Concilium catalogues the saving of time was estimated at one half a day, but in regard to other

cases the saving is much greater. If any zoologist familiar with the best bibliographical resources considers how he should go to work to ascertain what has been published in the past five years in regard to some comparatively minute question, such as the fauna of Sumatra, a minute's reflection will suffice to show that it would be a task of many weeks to obtain a complete answer to such a question. Yet a subscriber to the faunistic part of the bibliography of the Concilium would require only a few seconds to find the 85 publications dealing with the subject. Some of the latter, indeed, bear titles which would appear to preclude any reference to Sumatra and thus be likely to be missed by the student altogether. These 85 references would have cost the subscriber sixteen cents. Surely no argument is necessary to prove the value of the work nor the extreme cheapness of the service.

The general statement contains a key by which subscribers can verify their subscriptions and rest assured that they have received all that has been last published on any subject. The zoological and anatomical subjects include 760, 8,371 and 2,007 cards respectively during 1901. There are 263 cards on microscopic technique and 155 on general biology.

American subscribers will find it convenient to remit to Mr. Edwin S. Field, 427 Broadway, New York City, and also copies of the general statement for 1901 can be secured.

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## *SCIENTIFIC NOTES AND NEWS.*

Lord Kelvin is expected to arrive in New York on April 19. A reception will be given in his honor on the evening of April 21 by Columbia University, the American Institute of Electrical Engineers, the New York Academy of Sciences and other scientific societies.

Lord Lister and Professor Virchow are among those who have been elected honorary members of the Ghent Medical Society.

Dr. N. L. Britton, director of the New York Botanical Garden, expects to visit Cuba at the end of the present month, with a view to securing collections for the Garden. Dr. D. T. MacDougal, assistant director, is at present in Arizona and New Mexico, making collections, particularly of giant cacti.

Mr. William T. Palmer, of the U. S. National Museum, has been engaged in investigations of the natural history of Cuba.

Dr. Brandes has been appointed scientific director of the Zoological Gardens in Halle and has resigned his position as assistant in the zoological laboratory of the University.

Professor Hermann Kobold, astronomer in the Observatory at Strassburg, has removed to the Observatory at Kiel.

Professor H. Becquerel lectured in French before the Royal Institution on March 7. His subect was 'Radio-active Bodies.'

Dr. Simon Flexner, professor of pathology in the University of Pennsylvania, gave a lecture on March 18 before the Yale Medical Alumni Association on the subject 'Bubonic Plague.'

An address on 'Immunity' was delivered on March 7 before the students of Jefferson Medical College, Philadelphia, by Dr. W. H. Welch, of the Johns Hopkins University.

At the meeting of the Michigan Academy of Science to be held at the University of Michigan on March 27, 28 and 29, a public lecture will be given by Major Walter Reed, of the Army Medical Museum, at Washington, chairman of the U. S. Yellow Fever Commission. His subject will be 'Yellow Fever.'

C. N. Brown, professor of civil engineering in the Ohio State University and dean of the College of Engineering, died on March 6 from nervous prostration. He was forty-four years of age and had been connected with the Ohio State University as instructor and head of the department of civil engineering for the past twenty years.

Dr. Emil Selenka, honorary professor of zoology and comparative anatomy at the University of Munich, died on January 21.

The House Committee on Coinage has directed a favorable report to be made on the bill providing for the adoption by the United States of the metric system. It provides that after January 1, 1904, all the departments of the government, in the transaction of all busi-

ness requiring the use of weight and measurement, except in completing the survey of public lands, shall employ and use only the weights and measures of the metric system; and after January 1, 1907, the weights and measures of the metric system shall be the legal standard weights and measures of and in the United States.

THE Entomological Society of Western Pennsylvania was organized at the Carnegie Museum in Pittsburgh on the evening of March 8. Dr. W. J. Holland was elected president, Mr. F. A. Merrick, of New Brighton, secretary, and Dr. D. A. Atkinson, of Pittsburgh, Treasurer. Twenty-three persons participated in the organization. A committee with Herbert H. Smith as chairman was appointed to prepare a constitution and by-laws. The next meeting will be held on April 5.

THE thirteenth session of the International Congress of Americanists will be held in the halls of the American Museum of Natural History, New York City, October 20–25, 1902. The object of the congress is to bring together students of the archeology, ethnology, and early history of the two Americas, and by the reading of papers and by discussions to advance knowledge of these subjects. Communications may be oral or written, and in French, German, Spanish, Italian or English. All debates are expected to be brief, and no paper must exceed thirty minutes in delivery. The papers presented to the congress will, on the approval of the bureau, be printed in the volume of proceedings. Members of the congress are expected to send, in advance of the meeting, the titles and, if possible, abstracts of their papers, to the general secretary. The subjects discussed by the congress relate to: (1) The native races of America, their origin, distribution, history, physical characteristics, languages, inventions, customs and religions, and (2) The history of the early contact between America and the Old World. All persons interested in the study of the archeology, ethnology and early history of the two Americas may become members of the congress by signifying their desire to Mr. Marshall H. Saville, general secretary of the commission of organization, American Museum of Natural History, New York, and remitting either direct to the Treasurer (Mr. Harlan I. Smith, American Museum of Natural History), or through the general secretary, the sum of three dollars. The receipt of the treasurer for this amount will entitle the holder to a card of membership and to all official publications emanating from the thirteenth session of the congress. Mr. Morris K. Jesup is president and the Duke of Loubat vice-president of the commission of organization.

THE American Social Science Association will hold its general meeting in Washington, beginning Monday, April 21, and closing Friday, April 25. Dr. Oscar S. Straus, president of the association, will deliver his address on the first day. The program for April 22 will be devoted to the department of social economy, of which Mr. John Graham Brooks is chairman. The department of jurisprudence, of which Dr. Francis Wayland is chairman, will hold its sessions on the 23d. General George M. Sternberg, chairman of the department of health, will preside at the session on Thursday, April 24. Dr. W. C. Woodward and Professor George M. Kober will make addresses at the morning session and General Sternberg and Mr. Charles F. Weller will be the speakers at the evening session.

THE convocation of the University of the State of New York is to be held at Albany on June 30 and July 1. On Monday evening, June 30, Dr. Nicholas Murray Butler, president of Columbia University, will deliver the principal address, on 'Fundamental Principles of Education in the United States'; on Tuesday President Schurman of Cornell University will open a discussion on 'The Elective System and its Limitations.'

THE Jefferson Memorial and Interstate Good Roads Convention will be held at Charlottesville, Virginia, on April 2, 3 and 4 under the auspices of the Office of Public Road Inquiries of the Department of Agriculture, the National Good Roads Association and the Jefferson Memorial Road Association. The

Southern Railway good roads special train, carrying twenty-two engineers and road experts and equipped with fifteen car loads of the latest improved road-making machinery, will arrive at Charlottesville on March 24, and begin the construction of the Jefferson Memorial Road which will connect the home and tomb of Thomas Jefferson with the University of Virginia, which he founded.

The managers of the New York Botanical Garden have authorized the purchase in Berlin for $1,600 a collection of botanical works all dated prior to 1800.

The *Osprey* states that an interesting and valuable collection of northeast African birds has been recently received by the U. S. National Museum from Dr. A. Donaldson-Smith of Philadelphia, the well-known African explorer.

An anonymous gift of $20,000, for the benefit of the Harvard College Observatory, has been received from a friend of the director, Professor Edward C. Pickering, who in announcing the gift says: A very urgent need of the observatory will be relieved at once by this gift. The building provided, nine years ago, for the astronomical photographs, has become wholly inadequate to contain them, owing to their continual and rapid growth. It is proposed to expend about one half of this fund in extending the present building, by the erection of a wing to the east, which will provide for the adequate storing of this collection with its probable increase for many years. These photographs furnish a history of the entire stellar universe for the last twelve years, which is not duplicated elsewhere. Whenever a new object is discovered in any part of the sky, we are therefore able to study its past history during this period. Evidently, provision should be made for extensive use of this collection by large numbers of astronomers. A much larger building, staff and endowment, than our present means permit, would be required for this purpose. Accordingly, the new wing will be so constructed that when these plans are carried out, it can be used for holding the valuable collection of astronomical books (one of the most complete in the world) belonging to the observatory. These books are now contained in a wooden building fifty years old, and are in danger of destruction by fire at any time. The remainder of the fund will be expended from time to time as urgent needs occur. It is proposed to employ a portion of it at once in studying new objects of interest on the photographs, since without it we have hitherto only been able to examine those of special importance. The value of a fund which will provide for such emergencies must be obvious. The larger plans described above, I hope, indicate the healthy and insatiable appetite of an institution which is always attempting to reach out into untrodden fields, and in which each accession suggests opportunities of still further extending its work into the unknown.

The thirteenth session of the Biological Laboratory of the Brooklyn Institute of Arts and Sciences, located at Cold Spring Harbor, Long Island, will be held for six weeks beginning July 2, 1902. The following courses of instruction are announced: High School zoology, Drs. C. B. Davenport and S. R. Williams; comparative anatomy, Professor H. S. Pratt, of Haverford College; invertebrate embryology, Professor C. P. Sigerfoos, University of Minnesota; animal bionomics and variation, Dr. Davenport; investigation in zoology, by various instructors; cryptogamic botany, Professor D. S. Johnson, Johns Hopkins University; plant ecology, Mr. S. M. Coulter, Washington University, assisted by Louise B. Dunn, of Columbia University; seminar in the same; bacteriology, Professor F. N. Davis, Bucknell University; investigation in botany; microscopic methods, Mrs. Davenport; nature study, Mr. Roy S. Richardson, High School, Brooklyn, N. Y. Biological discussions, lectures and excursions are arranged for. The tuition is $25 for the use of the laboratory. Board and room cost $6 per week. The director of the laboratory, who may be addressed for further details, is Professor C. B. Davenport, the University of Chicago.

The Ohio State University announces for the 1902 session of its Lake Laboratory at Sandusky, on Lake Erie, courses in zoology, botany, entomology, ornithology, ichthyology,

and vertebrate and invertebrate morphology. The courses of lectures and laboratory instruction open on July 7 and continue for six weeks, but the opportunities of the laboratory are open to investigators from June 15 to September 15. Investigators qualified to carry on independent research work are given the facilities of the laboratory free of expense, but persons desiring this opportunity should apply to the director as early as convenient with statement of the time during which table room is desired. A detailed circular may be had on application to the director, Professor Herbert Osborn, Columbus, Ohio.

A TELEGRAM has been received at the Harvard College Observatory from Professor Hussey at the Lick Observatory stating that from a recent Crossley photograph Professor Perrine finds no evidence of polarization in condensations *A* and *D* of the nebula surrounding Nova Persei.

*Nature* states that the Russian Geographical Society has awarded this year its Constantine medal to the geologist, K. I. Bogdanovitch, who has spent several years in the exploration of Central Asia and has contributed one large volume to the beautiful series of quarto volumes edited by the Society and devoted to this part of Asia. The Semenoff medal has been awarded to Professor Eduard Suess for his new classical work, 'Das Antlitz der Erde,' and the Prjevalsky medal to the zoologist, Professor Zarudnyi, the author of several most valuable works on the birds and also the geography of the Transcaspian region, and the author of a work, 'Journey to East Persia,' just published by the Society. The great gold medal of the section of statistics has been awarded to N. V. Slyunin, for his researches into the economical conditions of the inhabitants of the Okhotsk and Kamchatka coasts. Three small gold medals have been awarded to Messrs. N. P. Petrovsky, D. K. Zelenin and M. N. Kositch for ethnographical works published in the excellent ethnographical periodical of the Society, *Zhivaya Starina (Living Antiquities)*. Professor Gordyaghin, of Kazan, has been awarded the Prjevalsky silver medal for his botanical work in East Russia, and the Semenoff silver medal has been awarded to A. K. Bulatovich for his journey to Lake Rudolph. A number of small silver medals have also been awarded, chiefly for meteorological work in connection with the Society's meteorological committee, or for expeditions.

*Nature* learns from the *Ceylon Observer* that Mr. Alexander Agassiz and his party have returned to Colombo, from their exploration of the Maldives. About three hundred photographs were taken, principally of coral-reef subjects. The principal work done was the sounding of the channels between the lagoons and the development of the plateau on which the atolls of the Maldives have been formed. The principal atolls are separated by comparatively shallow water in the central part of the group, while towards the south, between Hadumati and Suvadiva and Addu, the depths are very much greater—nearly a thousand fathoms. A line was run to the westward of Ari Atoll into fifteen hundred fathoms, and one to the southward of South Male into twelve hundred fathoms, showing that the plateau of the Maldives is much steeper on the west than on the east face. Soundings were also taken between the northern Maldives and Colombo, and they show that the Maldives are separated from the Indian continental slope by a deep bank of the ocean of more than fifteen hundred fathoms in depth. The atolls of the Maldives are said to exhibit the most simple and primitive conditions for the formation of atolls which are found anywhere except in some parts of the Yucatan plateau in the West Indies. Atolls can be found in all stages of growth, from a mere bank rising to a few feet above the plateau to banks within five or six fathoms from the surface or to banks which have just reached the surface and on which sandbanks or islets are beginning to form. Mr. Agassiz says that one reason for the success of his expedition is that the charts published more than seventy years ago are as accurate to-day as they were then. The only changes noticed were changes such as the washing away of banks or the formation of banks since the charts were published; but these are changes without any special importance.

*UNIVERSITY AND EDUCATIONAL NEWS.*

Mrs. Collis P. Huntington has given $250,000 to the Harvard Medical School to erect a laboratory of pathology and bacteriology in memory of the late Mr. Huntington. The sum of $821,225 has now been collected which makes available Mr. John D. Rockefeller's gift of $1,000,000. The donors to this fund whose gifts are $5,000 or over are as follows:

| | |
|---|---|
| Miss Mary S. Ames | $5,000.00 |
| Oliver Ames | 5,000.00 |
| C. W. Amory | 10,000.00 |
| Anonymous | 10,000.00 |
| Robert Bacon | 25,000.00 |
| Francis Bartlett | 10,000.00 |
| Mrs. S. Parkman Blake | 10,000.00 |
| John L. Bremer | 10,000.00 |
| Mrs. John L. Bremer | 5,000.00 |
| Miss Sarah Bremer | 5,000.00 |
| Walter C. Cabot | 5,000.00 |
| W. Murray Crane | 5,000.00 |
| George F. Fabyan | 25,000.00 |
| Mrs. William H. Forbes | 5,000.00 |
| Augustus Hemenway | 15,000.00 |
| Francis L. Higginson | 60,000.00 |
| George Higginson | 10,000.00 |
| Henry L. Higginson | 10,000.00 |
| James J. Higginson | 10,000.00 |
| H. H. Hunnewell | 12,500.00 |
| Mrs. Collis P. Huntington | 250,000.00 |
| Eben D. Jordan | 5,000.00 |
| David P. Kimball | 5,000.00 |
| Elliott C. Lee | 25,000.00 |
| Joseph Lee | 5,000.00 |
| Arthur T. Lyman | 5,000.00 |
| W. L. Richardson | 25,000.00 |
| Mr. and Mrs. Frederick C. Shattuck | 50,000.00 |
| David Sears | 25,000.00 |
| Francis Skinner | 5,000.00 |
| John T. Spaulding | 10,000.00 |
| W. S. Spaulding | 10,000.00 |
| James Stillman | 100,000.00 |
| Nathaniel Thayer | 25,000.00 |

We noted last week the gift of £25,000 of Mr. William Johnston to University College, Liverpool. We learn from *The British Medical Journal* that the £25,000 is divided as follows: £10,000 is allocated to found a chair of chemical biology, £6,000 at 5 per cent. interest to permanently endow three research fellowships of £100 a year each. Of these fellowships one is to be held by a medical graduate of a colonial university, a second by a graduate of medicine of the United States, and a third by a research student in gynæcology. The remaining £9,000 is to be spent in building a laboratory adjoining the Thompson-Yates laboratories, to accommodate the Tropical School, the professor of chemical biology, experimental medicine, comparative pathology, and serum research departments.

John D. Rockefeller has offered to give $25,000 to the endowment fund of William Jewell College, Liberty, Mo., provided $75,000 additional is raised by January 1, 1903.

The corner stone of the science and administration building of Colorado College has been laid. The building, to cost $225,000, will be three stories high, 287 feet long and 95 feet wide, the material being sandstone. A natural history museum will be installed in the building, with laboratories for scientific instruction.

The Laboratory of Chemistry and Metallurgy, at Lafayette College, the gift of Mr. James Gayley of the class of '76, will be dedicated on April 5. The program for the dedicatory exercises includes addresses by President Ira Remsen, of Johns Hopkins University; President Thos. M. Drown, of Lehigh University, and Professor Henry M. Howe, of Columbia University.

The Science Hall, University of Montana, was practically destroyed by fire on March 14. The building was erected three years ago at a cost of $100,000. The loss is covered by insurance.

At a conference of representatives of the governors of King's and Dalhousie colleges, held at Halifax, the joint committees unanimously arrived at a satisfactory basis of amalgamation for the federation of the various colleges in the maritime provinces, which will be submitted to the boards of governors of the various colleges for confirmation.

At Yale University, Dr. Wesley R. Coe, instructor in comparative anatomy, and Dr. Milton B. Porter, instructor in mathematics, have been appointed to assistant professorships.